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Three Essays on Occupational Safety and Labor Market Outcomes

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Abstract

This dissertation comprises of three papers on occupational safety and health. Occupational injuries and illnesses are prevalent and costly. To reduce workplace injuries and the associated costs, the government uses workplace inspections and the associated penalties as the primary enforcement tool. This dissertation examines the direct effect of the government enforcement on workplace injuries as well as the indirect effect on labor market outcomes and firm dynamics.

Chapter 1 examines the effect of workplace inspections on workplace safety, product quality, and worker productivity in nursing facilities. The identification strategy exploits the nationwide Site-Specific Targeting (SST) plan of the Occupational Safety and Health Administration (OSHA). The SST plan prioritized establishments for inspection if their injury case rates exceeded a threshold, generating a discontinuous increase in inspections at the SST threshold. The identification strategy exploits this discontinuous increase using a regression discontinuity design. The analysis sample is constructed by matching establishment-level data on injury case rates to OSHA inspection records and the quality measures and staffing levels from the Centers for Medicare and Medicaid Services (CMS). According to the data, the likelihood of inspections increases at the SST threshold by 32 percentage points. The discontinuous increase in inspections is associated with lower injury case rates of the nurses, but worse healthcare quality and lower nurse productivity. The results suggest improving occupational safety may come at the expense of service quality and worker productivity.

Chapter 2 (joint with Perry Singleton) examines the effect of workplace inspections on worker safety. The Occupational Safety and Health Administration (OSHA) enforces safety regulations through workplace inspections. We estimate the effect of inspections on worker

safety by exploiting a feature of OSHA's Site Specific Targeting plan. The plan targeted establishments for inspection if their baseline case rate exceeded a cutoff. This generated a discontinuous increase in inspections, which we exploit for identification. Using the fuzzy regression discontinuity model, we find that inspections decrease the rate of cases involving days away from work, job restrictions, and job transfers in the calendar year immediately after the inspection cycle. We find no effect for other case rates or in subsequent years. Effects are most evident in manufacturing and less evident in health services, the largest two-digit industries represented in the data.

Chapter 3 examines the effect of financial penalties on workplace safety and worker productivity in coal mines. The variation of the financial penalties comes from the introduction of "flagrant" violations in the Mine Improvement and New Emergency Response Act (MINER Act) of 2006. The flagrant violation may lead to a penalty of up to 0.22 million per violation. Using an event-study model, the results show that three to four years after the issuance of a flagrant violation, the injury rates of the miners decreased by a significant 20 percent and worker productivity decreased by 6 percent. The coal mines were 4 percentage points more likely to stop operating. The results suggest the monetary value of the productivity loss is 1.3 times as the costs saved from fewer injuries, which highlights the unintended costs of workplace safety regulations.

THREE ESSAYS ON OCCUPATIONAL SAFETY AND LABOR MARKET OUTCOMES

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Chapter 1. Helping Nurses or Hurting Patients: The Effect of Workplace Inspections in Nursing Facilities

1. Introduction

Workplace inspections and the associated penalties are the government's primary tools to reduce workplace injuries, which cost $206 billion annually in wage and productivity losses, medical expenditures, and administrative expenses (National Safety Council, 2015). While the goal of inspections is to reduce workplace injuries and the associated costs, improvements in safety may have an unintended effect on product quality and worker productivity. On one hand, improvements in safety may be achieved through enhanced production practice or technology, which may also increase product quality and worker productivity (Black and Lynch, 2001). However, improvements in safety may require additional effort devoted to compliance and precautions (Krueger, 1990), which may subsequently decrease product quality and worker productivity. Thus, the net effect of inspections on product quality and worker productivity is ambiguous.

This study provides empirical evidence on the effect of workplace inspections by the Occupational Safety and Health Administration (OSHA) on workplace safety, product quality, and worker productivity. The empirical analysis focuses on nursing facilities, one of the most dangerous industries with respect to workplace safety. In 2015, the 3.3 million workers employed in nursing facilities experienced on average 6.8 cases of workplace injuries or illnesses per 100 full-time equivalent employees, much higher than the 3.8 cases in manufacturing and the 3.3 cases as the national average (BLS, 2016a, BLS, 2016b). More importantly, these injuries come predominantly from providing direct care for residents. In particular, 44 percent of the injuries in health care facilities comes from patient handling and movement, and 37 percent comes from slips, falls, and trips (Gomaa et al., 2015). During the inspections in nursing facilities, OSHA identifies violations of both general safety standards and hazards specific to nursing facilities, including musculoskeletal disorders and slips, trips, and falls. Inspections and the associated financial penalties may incentivize the facilities to reduce injuries. However, effort to reduce injuries, such as adjustments in the practice of moving and handling patients, could directly affect the practice of healthcare in the inspected facilities.

Empirically, the challenge of identifying the causal effect of OSHA inspections is that inspections are not random. First, typically inspections are conducted more frequently in more dangerous firms (Kniesner and Leeth, 2014), generating a negative correlation between inspections and workplace safety. Second, inspections may be more frequently conducted in establishments with less efficient managers or lower quality workers, generating a negative correlation between inspections and product quality, and between inspections and worker productivity. These cross-sectional correlations would confound the causal effect of inspections on safety, quality, and productivity.

To overcome these concerns, this study exploits the design of OSHA's Site-Specific Targeting (SST) plan. The SST plan is the first nation-wide program that targeted establishments for inspection based on establishment-level injury case rate. From 1996, OSHA surveyed the annual workplace injury case rates of around 80,000 establishments each year through the OSHA Data Initiative (ODI). Based on the case rates reported in ODI, OSHA prioritized establishments for inspection if the case rates exceeded a threshold. Importantly, the SST threshold was selected only after collecting the injury case rates, preventing employers from manipulating their injury case rates to avoid inspection. By design, the SST plan generated a discontinuous increase in the likelihood of inspections at the SST threshold.

The identification strategy exploits the discontinuous increase in inspections at the SST threshold using a fuzzy regression discontinuity (FRD) design. The key identification assumption is that establishments with injury case rates right above and below the SST threshold are comparable. The assumption is examined by testing the smoothness of the distribution of the establishments and the establishment characteristics at the SST threshold. The FRD design uses the SST threshold as an instrument for whether an establishment has an inspection, which identifies the local average treatment effect among compliers with injury case rates close to the SST threshold.

To implement the FRD design, a unique establishment-level dataset is constructed by linking surveys on injury case rates to administrative records on inspections and a census of nursing facilities. The injury case rates of the facilities covered by the SST plan are from ODI. The inspection records are from OSHA's Integrated Management Information System (IMIS). The quality measures and staffing levels are from a census of the nursing facilities complied by the Centers for Medicare & Medicaid Services (CMS). The linked data include 13,592 facility-year observations, which provide a large representative sample for estimating the effect of inspections on worker safety and service quality.

According to the matched data of injury case rates and inspection records, the SST plan is associated with a 32 percentage point increase in the likelihood of inspections at the SST threshold. Moreover, the distribution of facilities is smooth at the threshold and the establishment characteristics are similar above and below the threshold, suggesting the identification assumption of FRD design is valid.

The estimates using the FRD design suggest that inspections improve workplace safety. After inspections, the number of cases involving days away from work, job restrictions or transfer (DART) is estimated to decrease significantly by 5.6 cases per 100 full-time equivalent employees, representing a 38 percent decrease compared with the average DART at the threshold. The results suggest OSHA inspections are effective in improving workplace safety in facilities with injury case rates close to the SST threshold.

While inspections improve workplace safety, they negatively affect the quality of care. First, inspections are associated with a 16.8 percentage point increase in deficiency citations on providing ADL care, representing a more than two hundred percent increase. Second, inspections are associated with a significant decrease in the number of residents receiving full assistance with ADLs. This may reflect that nurses avoid injuries by reducing ADL care, as patient handling and moving account for nearly half of the nurse injuries. The residents also show behavioral symptoms after inspections. Overall, the results imply a negative impact of inspections on the quality of care in nursing facilities.

The results also suggest that workplace inspections decrease worker productivity. The productivity of nurses is approximated using quality-adjusted output per labor hour (Sojourner et al., 2015).[1] After inspections, nursing facilities serve the same number of residents while the quality of care worsens, evidenced by lower quality of ADL care and worse health outcomes. Additionally, labor input, measured by the number of nursing hours per resident, does not change according to the staffing levels from CMS. Taken together, the results suggest that inspections have a negative impact on worker productivity.

[1] Only a few studies present empirical evidence on the productivity of health care personnel since the output, namely the healthcare provided to patients, is difficult to quantify. Previous empirical studies adopt different measures on productivity: Skinner and Staiger (2015) use one-year survival of the patients, Tong (2011) use mortality, and Bartel et al. (2014) use the length of stay in hospital. However, none of these measures directly take labor input into account.

This study contributes to several literatures. First, using a regression discontinuity design and a unique dataset, the results provide new evidence of the effect of workplace inspections on injury case rates. Most previous studies estimate the average effect of OSHA inspections in specific industries using various identification strategies [2] and often find a small and insignificant effect of inspections on the injury rates (Smith, 1979; McCaffrey, 1983; Bartel and Thomas, 1985; Ruser and Smith, 1991; Gray and Mendeloff, 2005).[3] However, even in dangerous industries, many of the inspected establishments have low injury rates and the inspections may not further reduce injuries (OSHA, 2004). Instead of focusing on all the inspected establishments, this study finds that inspections decrease the injury case rates significantly among establishments with case rates close to the SST threshold, suggesting that OSHA inspections are effective among relatively dangerous establishments.

This study also provides the first evidence on the trade-off between workplace safety and worker productivity in the service sector. Previous studies focus exclusively on firms in manufacturing, construction, and mining (Sider, 1983; Gray, 1987; Kaminski, 2001; Gowrisankaran et al., 2017). A close study to this paper is Gowrisankaran et al. (2017), which find fatal accidents in coal mines are associated with fewer injuries and lower miner productivity. Fatal accidents may affect worker productivity through channels not directly related to workplace safety, such as increased media exposure, temporary mine closures, and extensive

[2] For example, Bartel and Thomas (1985) use industry-level data and estimate the correlation between number of inspections and injury rates; Smith (1979) and Ruser and Smith (1991) use establishment-level data and compare injury rates of plants inspected early and late in a given year; and Gray and Mendeloff (2005) estimate the change of injury rates at establishment level before and after inspections. Two exceptions are Li and Singleton (2017) and Peto et al. (2016), both of which use the SST plan to identify the effect of OSHA inspections.

[3] The exceptions include Levine et al. (2012), and Li and Singleton (2017), which find OSHA inspections reduce workplace injuries significantly.

safety enforcement. The advantage of this study is that the variation of safety is derived from regular workplace inspections, which are less likely to cause dramatic changes in factors other than the enforcement of safety standards.

Lastly, this study highlights the unintended effect of nurse safety regulations on healthcare quality. Considerable research has shown the important role of nurses in providing high quality health care. Factors such as the number of nurses (Lin, 2014), the composition of the nursing team (Bartel et al., 2014), and the pay regulation of nurses (Propper and Van Reenen, 2010) affect the quality of care and patient outcomes significantly. As nurses get injured mostly from providing direct care for residents, regulations aimed at reducing workplace injuries among nurses are likely to have a negative impact on the quality of care provided for the patients.

The rest of this paper proceeds as follows. Section 2 provides the background on OSHA inspections and the Site-Specific Targeting (SST) plan of OSHA. Section 3 presents the data and descriptive statistics. Section 4 discusses the empirical method. Section 5 presents the results and Section 6 concludes.

2. OSHA Inspections and Site-Specific Targeting Plan

The Occupational Safety and Health Administration (OSHA), created after the passage of the Occupational Safety and Health Act of 1970, is a federal agency whose mission is to assure safe and healthful working conditions for workers. OSHA developed a series of workplace health and safety standards that most firms are obliged to obey.[4] To enforce these standards, OSHA conducts about 80,000 inspections annually.

[4] The exceptions are some public sector employers and workers. Federal OSHA plan only covers employers and worker in the private sector. Twenty-six states have their own state plans to cover employers and workers in the public sector.

OSHA inspections are likely to improve the workplace safety for multiple reasons. First, OSHA always conduct inspections without advance notice[5], making it difficult for firms to act strategically before the inspections. Second, most inspections lead to citations on violations of safety and health standards: 62 percent of the inspections find at least one violation, and 58 percent of these violations are deemed severe by OSHA.[6] OSHA may levy penalties for these violations up to $12,934 per violation. OSHA also mandates firms to correct the violations within a time limit. Additionally, inspections increase the costs of future violations: the penalty for each repeated violation is up to $129,336. Beyond detecting violations, inspections raise managerial attention to general occupational safety issues not directly related to violations found in inspections (Mendeloff and Gray, 2005). Overall, OSHA inspections provide incentives from various aspects for firms to improve safety conditions and reduce workplace injuries.

OSHA inspections fall into two general categories: programmed inspections or unprogrammed inspections. Programmed inspections, constituting 56 percent of OSHA inspections, are conducted based on establishment industry, potential hazards, or injury case rates, and are mostly complete inspections of all the potential hazards. Unprogammed inspections are conducted based on employee complaints, accidents, or referrals. Unprogrammed inspections only focus on hazards specific to the incident.

To identify the effect of OSHA inspections, this study exploits the design of OSHA's Site-Specific Targeting (SST) plan. The SST plan is the first nation-wide program that conducts comprehensive inspections based on establishment-level injury case rates (OSHA, 2004).

[5] OSHA may give notices for special circumstances, usually less than 24 hours in advance. In the analysis sample, only 0.4 percent of the programmed inspections were noticed in advance.
[6] Author's calculation based on the inspections from 1999-2014. Data are from OSHA's Integrated Management Information System (IMIS).

Starting from 1996, OSHA used its annual OSHA Data Initiative (ODI) survey to collect establishment-level injury case rates. OSHA requires most firms to keep a log of all recordable workplace injuries.[7] In each year, OSHA selected about 80,000 establishments in industries with historically higher injury rates[8] and required the employers to report the total number of cases (TCR) and number of cases involving days away from work, job transfers or restrictions (DART) per 100 full-time equivalent employees.[9] While the injury case rates were self-reported by the employers, OSHA has rigorous standards on record-keeping and falsifying records could result in a criminal fine of $10,000 or up to 6 months in jail, or both.

After collecting data on injury case rates, OSHA selected the DART case rates to be used as the targeting thresholds for different industries[10] and prioritized establishments for inspection if the DART case rates exceeded the corresponding targeting threshold. The thresholds were selected based on the anticipated total number of inspections that OSHA would be able to conduct in the next cycle and the distribution of the DART case rates among the surveyed establishments. The thresholds were updated annually. The inspections were conducted during the SST inspection cycle, which started from around one year and a half after the initial

[7] OSHA recordable injuries include any work-related fatality; any work-related injury or illness that results in loss of consciousness, days away from work, restricted work, or transfer to another job; and any work-related injury or illness requiring medical treatment beyond first aid.
[8] The industries include manufacturing and non-construction industries with injury rates above the national average, selected based on industry level rate of nonfatal occupational injuries and illnesses from Bureau of Labor Statistics. 60 percent of the establishments in ODI are in manufacturing, 15 percent in services, 11 percent in transportation and communications, 8 percent in wholesale trade, and 5 percent in retail trade.
[9] Starting from 2002, number of cases with days away from work (DAFWII) per 100 employees is also collected in ODI.
[10] The SST plan had different thresholds targeting establishments in manufacturing, nursing and long-term care, and others. Starting from 2004, DAFWII case rate is added as an additional factor used to select the target list. However, about 90 percent of establishments on the target list have DART case rates above the SST threshold of DART.

collection of case rates and lasted for around one year. Table 1 shows the starting and closing dates of the SST plan from 2004-2011.[11] For example, ODI 2003 collected the establishment injury case rates in 2002, which were used to design SST plan 2004. The inspections of SST plan 2004 were conducted from April 2004 to Aug 2005. Thirty-five states participated in the SST plan, and the rest of the states have their own state plans on occupational safety and health.[12]

This study focuses on inspections among nursing facilities, which were first included in the SST plan in 1999, removed from 2000-2003 and added back since 2004. Figure 1 shows the DART thresholds that the SST plan used to target nursing facilities and the average DART case rates of facilities surveyed by ODI from 2004 to 2011. About 10 percent of the nursing facilities have DART case rates above the SST threshold. The inspections conducted in nursing facilities focus on the general OSHA standards as well as the specific safety and health hazards in the health service sector. These hazards include musculoskeletal disorders related to patient or resident handling, workplace violence, blood-borne pathogens, tuberculosis, and slips, trips and falls as defined by OSHA guidelines (OSHA, 2015).

3. Methodology

The main empirical objective of this paper is to estimate the causal effect of inspections on workplace safety, healthcare quality, and worker productivity in nursing facilities. The effect is defined by the following equation:

$$\tau_{ijt} = Y_{ijt+1}(S_{ijt} = 1) - Y_{ijt+1}(S_{ijt} = 0) \tag{1}$$

[11] The OSHA Data Initiative (ODI) has been suspended since 2011 and the SST plan since 2014.
[12] The states with their own plans are not covered by most of the federal OSHA programs. To obtain approval from OSHA for its own state plan, a state must go through extensive procedures. The majority of the state plans were initially approved in the 1970s to 1980s.

Y_{ijt+1} indicates the outcomes of nursing facility i in state j year $t+1$; S_{ijt} indicates whether the nursing facility receives an inspection in year t. The effect of an inspection is defined as the difference between the outcome when the facility with an inspection and without an inspection. Since $Y_{ijt+1}(S_{ijt}=1)$ and $Y_{ijt+1}(S_{ijt}=0)$ could not be observed at the same time, this paper uses a fuzzy regression discontinuity design to identify τ_{ijt}.[13]

The identification exploits the design of OSHA's Site-Specific Targeting (SST) plan. The key feature of the SST plan is that it increases the likelihood of inspections right at the SST threshold:

$$\lim_{X_{ijt}\downarrow 0} E[S_{ijt}|X_{ijt}] > \lim_{X_{ijt}\uparrow 0} E[S_{ijt}|X_{ijt}] \tag{1}$$

The running variable X_{ijt} is defined as $DART_{ijt} - SST_t$, the difference between the DART case rate and the corresponding SST threshold. The likelihood of inspections among establishments with DART case rates above the SST threshold is higher than the likelihood among those right below the threshold. Using this discontinuous increase in inspections, the effect of inspections, τ_{ijt}, is given by the following estimand:

$$\tau_{ijt} = \frac{\lim_{X_{ijt}\downarrow 0} E[Y_{ijt}|X_{ijt}] - \lim_{X_{ijt}\uparrow 0} E[Y_{ijt}|X_{ijt}]}{\lim_{X_{ijt}\downarrow 0} E[S_{ijt}|X_{ijt}] - \lim_{X_{ijt}\uparrow 0} E[S_{ijt}|X_{ijt}]} \tag{2}$$

The denominator measures the discontinuous change in inspections at the SST threshold. The numerator measures the discontinuous change in the outcomes of nursing facilities at the SST threshold. The fuzzy regression discontinuity design gives the local average treatment effect (LATE) of inspections among the compliers with injury rates close to the SST threshold. While

[13] Lee and Lemieux (2010) provides a review the regression discontinuity design.

the estimate may not be generalized to nursing facilities with lower injury rates, the effect of inspections among these relatively dangerous facilities is of the most policy interest.

The effect of inspections is estimates using the following three models. First, the first stage model estimates denominator of equation 2, which reflects the discontinuous increase in inspections among facilities with DART case rate at the SST threshold. Specifically, the first stage model is as follows:

$$S_{ijt} = \alpha_0 + \alpha_1 T_{ijt} + \alpha_2 f(X_{ijt}) + \alpha_3 T_{ijt} f(X_{ijt}) + \alpha_4 Z_{ijt} + \delta_j + \theta_t + \epsilon_{ijt} \quad (3)$$

The outcome S_{ijt} indicates whether nursing facility i in state j has any inspection during the SST plan corresponding to year t, which starts from the middle of the second year after collecting the injury case rates and lasts for around one year. T_{ijt} is defined as $1\{X_{ijt} \geq 0\}$, which is an indicator of whether the DART case rate of facility i is above the SST threshold. $f(X_{ijt})$ and $g(X_{ijt})$ are flexible controls of the DART case rates, allowed to be different across the SST threshold. Z_{ijt} includes control variables on the total number of beds, whether the facility is in a chain, and whether it is for-profit. The model also includes state and year fixed effects, δ_j and θ_t.

The coefficient of T_{ijt}, α_1, identifies the effect of the SST plan on the likelihood of inspections among facilities at the SST threshold. By design, α_1 should be positive and significant.

Second, the reduced form model estimates the numerator of equation 2, which reflects the discontinuous change in the outcomes of nursing facilities at the SST threshold.

$$Y_{ijt+1} = \beta_0 + \beta_1 T_{ijt} + \beta_2 f(X_{ijt}) + \beta_3 T_{ijt} g(X_{ijt}) + \beta_4 Z_{ijt} + \delta_j + \theta_t + \epsilon_{ijt} \quad (4)$$

Y_{ijt+1} indicates the outcomes of facility i one year after the corresponding SST inspection cycle. The right hand side of the model is the same as the first stage. The coefficient of T_{ijt}, γ_1, identifies the differential change in the outcomes of nursing facilities at the SST threshold.

Lastly, the causal effect of inspections on the outcomes of nursing facilities is modeled using the following equation:

$$Y_{ijt+1} = \gamma_0 + \gamma_1 S_{ijt} + \gamma_2 f\left(X_{ijt}\right) + \gamma_3 T_{ijt} g\left(X_{ijt}\right) + \gamma_4 Z_{ijt} + \delta_j + \theta_t + \epsilon_{ijt} \qquad (5)$$

The endogenous variable on inspection, S_{ijt}, is instrumented with T_{ijt}, the indicator of DART case rate above the SST threshold. The two-stage estimate of γ_1 gives the causal effect of OSHA inspections on the outcomes of nursing facilities.

The model is estimated using local linear regressions, first suggested by Hahn, Todd, and van der Klaauw (2001). Specifically, the optimal bandwidth is selected following the method suggested by Calonico, Cattaneo, and Titiunik (2014) and the standard errors presented are bias-corrected robust standard errors clustered at the facility level.[14] The advantage of estimating the model non-parametrically is that there is no need to specify functional forms of $f\left(X_{ijt}\right)$ and $g\left(X_{ijt}\right)$. If the functional forms are specified incorrectly, the estimates are likely to be biased. Additionally, it is common to use high-order polynomials as proxies of the functional forms, which leads to poor inferences (Gelman and Imbens, 2014).

[14] Calonico, Cattaneo, and Titiunik (2014) finds using a data-driven, asymptotically mean-squared error (MSE) optimal bandwidth and including a robust bias-correction term in the estimated confidence interval offer good finite-sample performance compared with commonly used approach that assumes away the bias of the estimator.

4. Data

4.1. Data Sources

This study uses establishment-level data linking the injury case rates to OSHA inspection records and a census of nursing facilities from CMS. The data on injury case rates are from the OSHA Data Initiative (ODI). ODI includes annual surveys covering about 80,000 establishments from 1996 to 2011. The establishments are sampled annually from those with 40 or more employees[15] in 46 states.[16] ODI contains basic information on the establishments, including name, street address, and industry. The injury case rates reported in ODI include Total Case Rate (TCR) and Days Away, Restricted, and Transfer (DART) case rate. Nursing and personal care facilities are oversampled in ODI. From 1996 to 2011, 143,771 surveys were conducted on 23,917 nursing facilities.

To determine to the effect of the SST plan on the frequency of inspections, the injury case rates from ODI are matched to the inspection records from OSHA Integrated Management Information System (IMIS). IMIS contains records on all closed OSHA inspections since 1970. The data include establishment name and street address, which are used to match the inspection records to the injury case rates from ODI. The data also include the inspection type, open and close dates of the inspection, which are used to determine whether an inspection is conducted under the SST plan and which year of the SST plan. Additionally, the data provide a detailed list on the violations and the amount of penalty associated with each violation, if applicable.

To estimate the effect of inspections on the quality of care in nursing facilities, the ODI/IMIS data are further matched to a census of the nursing facilities complied by the Centers

[15] In 1996 and 1997, only establishments with 60 or more employees were included.
[16] States did not participate in ODI in 2011 include Alaska, Oregon, South Carolina, Washington, Wyoming, and District of Columbia. These states have their own state plans

for Medicare & Medicaid Services (CMS), based on establishment name and address. The records on nursing facilities are mainly derived from the Online Survey, Certification and Reporting (OSCAR) database. OSCAR is the most comprehensive dataset at the facility level, containing information on operational characteristics, resident health outcomes, staffing level, and records on deficiency citations issued by state health agencies. The data are collected annually on average, with a standard window between 9 to 15 months (Harrington et al., 2015). The data include about 16,000 Medicare and/or Medicaid certificated nursing facilities each year, representing more than 95 percent of long-term care facilities in the US. The empirical analysis uses data from 2006 to 2011, since from July 2012 the system is transited to Certification and Survey Provider Enhanced Reports (CASPER) and some of the health outcomes are no longer available.

The quality of care in nursing facilities is approximated by the quality of assistance with activities of daily living (ADLs) and the resident health outcomes. Two measures on the quality of assistance with ADLs are considered. The first is the number of deficiency citations on providing appropriate ADL care, which reflects the results of annual onsite evaluations conducted by state health agencies. State health agencies conduct annual examinations on whether a facility is in compliance with more than 100 federal requirements regarding quality of care, quality of life, and facility practices. The deficiencies regarding ADL care includes violations of the following standards: “activities of daily living do not decline unless unavoidable”, “resident is given treatment to improve abilities”, and “activities of daily living care is provided for dependent residents”. The second set of measures of assistance with ADLs is the fraction of residents receiving full assistance from staff to transfer, use toilets, and eat. These variables are reported by staff and reflect assessment on the actual level of assistance provided to the residents during a seven-day period (CMS, 2008).

In addition to the quality of ADL care, four health outcomes are used to measure healthcare quality. Contractures reflect a restriction of full passive range of motion of any joint due to deformity, disuse, and pain; pressure sores reflect the skin integrity of residents, unplanned weight changes reflect any unplanned weight gain or loss of 5 percent in one month or 10 percent over six months; and behavioral symptoms include a wide range of behaviors that are harmful to the residents themselves or disruptive in the environment, including wandering, verbally or physically abusive, socially inappropriate or disruptive, and resistive to care. These four health outcomes are selected as they are frequently used to measure quality of care in nursing homes and are also sensitive to the quality of nursing care.

4.2. Analysis Sample

The main analysis sample includes nursing facilities surveyed by ODI from 2002 to 2007. These facilities are covered by the SST plan from mid-2004 to mid-2010 and the outcomes are from 2006 to 2011, measured around one year after the end of the SST inspection cycle. Facilities with fewer than 10 residents are excluded. The main analysis sample includes 13,593 nursing facility-year observations.

Table 2 shows the descriptive statistics of the analysis sample as well as the subsample with DART case rates right above and below the SST threshold. The nursing facilities have on average 10.68 injuries per 100 full-time equivalent employees (TCR) annually, among which 6.98 cases involves days away from work, job transfers or restrictions (DART). While only 4.4 percent of the whole analysis sample is inspected, the SST plan dramatically increases the inspection likelihood among facilities with DART above the threshold. Among facilities with

DART from 0 to 5 cases above the SST threshold, 39 percent of them receive an inspection during the SST inspection cycle, much higher than the 3 percent among those within 5 units below the threshold.

To examine the effect of inspections on workplace safety, a subsample is constructed consisting of facilities with multiple surveys from ODI. The injury case rates are only observed if a facility is surveyed in ODI. As ODI selected a different sample of establishments each year, facilities were typically surveyed several times, but not every year. Among the main analysis sample, included are those with another survey four year after the initial survey, which is around one year after the SST inspection cycle. This sample includes 4,707 facility-year observations.

The key assumption of the regression discontinuity design is that firms right above and below the SST threshold should have similar observed and unobserved characteristics. The assumption is likely to be valid based on the design of the SST plan. OSHA selected and announced the SST threshold after collecting the data on injury case rates and updated the threshold every year, making it difficult to precisely predict the threshold ex-ante. Thus, nursing facilities should have limited ability to manipulate their injury case rates and avoid inspections. Figure 2 shows the distribution of nursing facilities by DART case rates relative to the SST threshold using the main analysis sample. Consistent with the assumption, the distribution shows no discontinuous change across the SST threshold. The density test suggested by McCrary (2008) gives a log density of 0.026 and standard error of 0.092, confirming that the distribution is smooth across the SST threshold.

5. Results

5.1. The SST plan and Inspections

The SST plan prioritized nursing facilities for inspection if the DART case rates exceeded the SST threshold. To examine the magnitude of the SST plan graphically, Figure 3, Panel A plots the frequency of inspections by DART case rate relative to the SST threshold. The inspections include any programmed inspections conducted during the corresponding SST inspection cycle. The lines in Figure 3 show the fitted values using local linear smoothing. Visually, the frequency of inspections shows a sizable increase at the SST threshold: 39 percent of the nursing facilities with DART case rates within 1 unit above the threshold receives an inspection during the SST inspection cycle, and only 6 percent of those within 1 unit below is inspected.

The first-stage results, estimated using equation 3, are presented in Table 3, Panel A. Column 2 reports the mean of the dependent variable right at the SST threshold. Column 3 reports the estimates of the discontinuity at the SST threshold using local linear regressions, with state and year fixed effects and controls on the number of beds, whether the facility is in a chain, and whether for-profit. The SST plan increases the frequency of inspections by 32 percentage points, representing a five hundred percent increase compared with the average frequency of inspections among facilities right below the threshold. The SST plan also increases the frequency of any violations of safety standards by 25 percentage points, suggesting many OSHA inspections identify some violations of safety standards.

While the SST plan creates a discontinuous increase in the frequency of inspections, using the discontinuity to identify the causal effect of inspections requires facilities near the SST threshold to be similar. To test this assumption, first, the frequency of inspections in the year right before and after the SST inspection cycle is examined. While the SST plan dramatically increases the frequency of inspections at the SST threshold during the SST plan inspection cycle, any differential changes at the threshold before or after the inspection cycle will bias the estimates on the causal effect of inspections. The graphical evidence is presented in Figure 3, Panel C and D. Consistent with the assumption, the frequency of inspections in the year before and after the SST inspection cycle is relatively low and shows no sizable change at the SST threshold. The estimated differences are small and statistically insignificant (Table 3, Panel B). Second, the differences of the operational characteristics at the SST threshold are examined, including the number of beds, the number of residents, whether the facility is in a chain, and whether it is for-profit. The tests reveal no selection of nursing facilities as these observed characteristics show small and insignificant changes at the threshold (Table 3, Panel C).

5.2. Inspections and Workplace Injuries

Clearly, nursing facilities with DART case rates above the SST threshold are similar to those below the threshold, except for the higher frequency of inspections. To examine the effect of OSHA inspections on injury case rates, Figure 4 plots the injury case rates one year after the SST inspection cycle by DART case rate relative to SST threshold. While both DART and TCR one year after the SST plan are positively correlated with DART in the initial survey year, both measures show a discontinuous decrease right at the SST threshold. As nursing facilities with DART above the SST threshold are more likely to be inspected, the discontinuous decrease in DART and TCR at the SST threshold suggests that inspections are associated with lower injury case rates.

Table 4, column 3 presents the reduced form estimates, which measure the size of the discontinuity at the SST threshold. The estimates from the reduced form equation 4 suggest that facilities right above the SST threshold have 1.30 fewer injuries involving days away from work, job transfers or restrictions and 2.06 fewer injuries of any type per 100 employees. Column 4 presents the two-stage estimates of equation 5 using the SST threshold as an instrument of the inspection variable. After an inspection, DART decreases by 5.6 cases per 100 employers, representing a 38 percent decrease among nursing facilities close to the SST threshold. TCR case rate decreases by 7.3 cases per 100 employees (38 percent). Both DART and TCR decrease by a similar proportion, suggesting that inspections reduce both mild injuries with no losses of workdays and relatively severe injuries with losses of workdays. Overall, the results imply that OSHA inspections are effective in reducing workplace injuries among relatively dangerous nursing facilities.

5.3. Inspections and Healthcare Quality

Inspections are found to be associated with fewer workplace injuries, but they may negatively affect the quality of healthcare in nursing facilities. As a highly labor-intensive industry, labor accounts for 74 percent of the total costs in nursing facilities (Gertler and Waldman, 1992). After inspections, nurses may devote extra effort to complying with OSHA regulations and preventing injuries, resulting in less effort on patient care and lower healthcare quality. Two sets of indicators on healthcare quality are examined: the quality of assistance with activities of daily living (ADLs) and the health outcomes of the residents.

Assistance with ADLs is particularly relevant in studying the association between nurse injuries and service quality. ADL care is the most fundamental care provided in nursing facilities, with 86 percent of the residents in need of assistance with at least one ADL.[17] ADL care also constitutes the major job responsibility of nursing aides, accounting for 63 percent of

[17] Author's calculation based on 13,507 residents from 2004 National Nursing Home Survey.

the staff in nursing facilities.[18] More importantly, assistance with ADLs involves extensive patient handling and moving activities, which contributes to nearly half of the workplace injuries in health care facilities (Gomaa et al., 2015). Thus, after inspections, facilities may adjust the practice of ADL care, as part of the effort to reduce workplace injuries.

The first indicator examined on ADL care is any deficiency citations on providing appropriate ADL care. Figure 5, Panel A plots the frequency of citations on providing ADL care by DART relative to the SST threshold. Facilities with DART case rates above the SST threshold, which are more likely to have an OSHA inspection, show an around 5 percentage point discontinuous increase on citations regarding ADL care. The estimates are shown in Table 5, Panel A. After inspections, the facilities are 16.8 percentage points more likely to have a citation on ADL care, representing a more than two hundred percent increase, compared with the mean frequency of 8 percent at the SST threshold. The results are consistent with the assumption that after inspections nurses reduce risky activities involving moving and handling patients to avoid workplace injuries. As a placebo test, deficiency citations on keeping clinical records, the most common citation, is examined. Nursing facilities are required to "keep accurate, complete, and organized clinical records on each resident that meet professional standards". Complying with the requirement on record-keeping is unlikely to cause workplace injuries and should not be affected by effort to reduce injuries. As expected, no discontinuous change shows in the number of citations on recording-keeping at the SST threshold after inspections (Figure 5, Panel B). The estimates suggest a small and insignificant decrease of deficiencies on record-keeping (Table 5, Panel A).

[18] Author's calculation based on nursing facilities in the Online Survey, Certification and Reporting (OSCAR) database from 2006-2011.

The second set of indicators on assistance with ADLs is the fraction of residents receiving full ADL assistance. As shown in Figure 6, after inspections, the fraction of residents receiving full assistance from staff to transfer, to use toilets, and to eat decreases discontinuously at SST threshold. Since the SST plan leads to no differential change in the number of residents across the threshold, the results suggest that nurses provide ADL assistance to fewer residents after inspections. Specifically, one year after an inspection, the fraction of residents receiving full assistant from staff to transfer decreases by 4.2 percentage points (18 percent), the fraction with full assistance to use toilets decreases by 6.9 percentage points (25 percent), and the fraction with full assistance to eat decreases by 3.6 percentage points (25 percent) (Table 5, Panel B). Overall, after inspections, facilities provide less assistance on ADLs.

In addition to ADL care, the quality of care is measured by the health outcomes of the residents, which are widely used to approximate the quality of care in studies on nursing home quality (Matsudaira, 2014; Lin, 2014; Bowblis and McHone, 2013). Figure 7 plots the resident health outcomes by DART case rates relative to the SST threshold. After inspections, three of the health outcomes, including the fraction of residents with contractures, pressure sores, and significant weight losses or gains, show no differential changes across the SST threshold. The exception is the fraction of residents with behavioral symptoms, which increases discontinuously at the threshold. The inspections are associated with an 8.5 percentage point increase in the fraction of residents who have behavioral symptoms, representing a 30 percent increase (Table 6). The effect of inspections on the fraction of residents with contractures, pressure sores, and unplanned significant weight change is small and insignificant. In summary, inspections are associated with worse quality of care, evidenced by lower quality of ADL care and more behavioral symptoms among residents.

5.4. Inspections and Patient Composition

After inspections, the quality of ADL assistance worsens, which is likely due to the effort to preventing injuries from moving and handling patients. Alternatively, nursing facilities may select patients in need of less ADL assistance after inspections, which will lead to fewer nurse injuries from moving patients and fewer residents receiving ADL care.

Little evidence supports the hypothesis of patient selection. First, nursing facilities can only discharge or transfer residents in a limited number of scenarios, including the closure of a facility, lack of payment for the service, improvement of health that nursing home care is not necessary or deterioration of health that nursing home care is not sufficient. Thus, it is difficult for the nursing facilities to manipulate the composition of the residents, especially in the short run. Additionally, the outcomes in the previous analysis are measured one year after the SST plan while the average length of stay in nursing facilities is 835 days and the median is 463 days.[19] Within one year, the limited turnover of residents suggests the results are unlikely to be driven by patient selection.

Second, the number of deficiencies regarding patient transfer and discharge shows little changes after inspections. Selecting easier residents may lead to more citations regarding patient transfer and discharge. Figure 8, Panel A presents the frequency of deficiency citations regarding residents transfer and discharge, including “no transfer or discharge without adequate reasons”; “providing timely notification and written records on transfer or discharge”; and “preparing each resident for a safe and easy discharge or transfer”. The lack of any significant changes in the

[19] Author’s calculation based on 12,973 residents surveyed in 2004 National Nursing Home Survey.

frequency of citations on patient transfer and discharge at the SST threshold suggests that facilities are unlikely to selectively transfer or discharge residents after inspections.

Lastly, the share of residents financed through Medicaid shows no change after inspections. Medicaid residents generally have lower reimbursement rates and worse health outcomes (Cohen and Spector, 1996). If facilities actively select easier patients after inspections, they are likely to selectively transfer or discharge the less profitable Medicaid residents. No change in the share of Medicaid residents appears at SST threshold after inspections (Figure 8, Panel B, and Table 7), which also suggests the worse quality of ADL care are unlikely to be driven by patient selection.

5.5. Inspections and Worker Productivity

Thus far, the results show that OSHA inspections reduce workplace injuries, but negatively affect healthcare quality, likely to be a result that nurses devote more effort to preventing injuries after inspections. With more effort devoted to preventing injuries, nurse productivity may also decrease. Nurse productivity is approximated by both the quality-adjusted care per unit of labor input (Sojourner et al., 2015). After inspections, the quality of care decreases with no change in number of residents, the remaining question is the effect of inspections on labor input.

The labor input is measured by the number of nursing hours per patient day among four types of nurses. In nursing facilities, about 63 percent of the staff are nursing aides, who typically assist residents with daily activities such as eating, dressing, and using the bathroom; 22 percent are licensed practical nurses, who provide direct care to residents under the supervision of registered nurses; 10 percent are registered nurses, who assess the health conditions of the residents and create personal care plans for each person; and 5 percent are nurses with administrative duties, who coordinate with staff but do not provide direct care for the residents.

Figure 9 plots the staffing level by DART case rate relative to the SST threshold one year after the SST plan. The nursing hours per patient day among nurses interacting directly with residents, including nursing aides, licensed practical nurses, and registered nurses, are similar across the SST threshold, as presented in Panel A-C. Table 8 shows the estimates on the effect of inspections on nursing hours per patient day. Inspections lead to small and insignificant changes in hours of nursing aides, licensed practical nurses and registered nurses. Thus, the less assistance with ADLs after inspections are unlikely to be a result of fewer nurses providing direct care for residents.

An exception is the hours of nurses with administrative duties, which increase after inspections, shown in Figure 9, Panel D. The hours of nurses with administrative duties increase by 0.08 hours per patient day, representing a 27 percent increase compared with the 0.28 hours per patient day on average. As nurses with administrative duties implement nursing policies and oversee other nurses, the results may suggest that facilities devote more effort to management and coordination of care and after inspections.

Overall, inspections have a small and insignificant impact on nursing hours devoted directly on patients but lead to worse quality of care, particularly on ADLs. The results reveal two potential mechanisms. After inspections, nursing facilities provide full assistance on ADLs to fewer number of patients to reduce injuries related to moving and handling patients. Additionally, nursing facilities might devote more labor to each task involving patient handling and moving to reduce injuries, as the availability of more caregivers are related with fewer musculoskeletal injuries (Trinkoff et al., 2003). The two mechanisms together contribute to a decrease in quality of ADL care and number of nurse injuries with no change in total nursing hours.

Considering nurse productivity approximated by quality-adjusted output per labor hour, while there is no change in labor hour and the number of residents served, the worse quality of care after inspections suggest lower nurse productivity after inspections. The results highlight the unintended effect of safety regulations on worker productivity. Consistent with previous literature in mining and manufacturing (Sider, 1983; Gray, 1987; Kaminski, 2001; Gowrisankaran et al., 2017), this study finds effort to improve workplace safety lead to lower worker productivity in nursing facilities.

5.6. Placebo Tests

The empirical evidence suggests after inspections nurses provide less ADL care and residents show more behavioral symptoms. One concern is the results may be driven by pre-inspection differences in outcomes at the SST threshold. To address this concern, the resident outcomes in the pre-SST periods are examined. The analysis sample includes nursing facilities covered by the SST plan 2007 to 2009. The pre-SST period is defined as the year of the initial survey on injury rates, which is around two years before the inspections. In the pre-SST period, the differences at the SST threshold are small and statistically insignificant among all resident outcomes, shown in Table 9, column 1. Column 2 presents the results on outcomes one year after the SST plan using the same sample. Facilities above the SST threshold have significantly more deficiencies on ADL care, with fewer residents receiving staff assistance with ADLs. The post-SST results are consistent with the estimates using the full sample (SST Plan 2004 to 2009), presented in Table 5 and 6. Overall, the results are unlikely to be driven by pre-inspection differences among resident outcomes.

Another placebo test considers states not participating the SST plan. The SST plan covers nursing facilities in thirty-five states and the rest of the states have their own state plans on occupational safety and health. These state plans often include programs enforcing the safety and health standards in nursing facilities, but do not use the SST threshold to select the target list. Thus, the resident outcomes should show no discontinuity at the SST threshold in facilities in states with their own OHSA plans. Column 3 shows the results on ten states that have state OSHA plans and were surveyed in ODI. As expected, resident outcomes show small and insignificant changes at the SST threshold.

6. Conclusion

This study measures the effect of OSHA inspections on the workplace, healthcare quality, and worker productivity in nursing facilities. The inspections reduce workplace injuries among the nurses, but negatively affect the quality of care, evidenced by worse quality of ADL care and more behavioral symptoms among the residents. The worse ADL care quality may be a result that nurses avoid injuries by reducing patient handling and moving activities. The results also imply a decrease in worker productivity after inspections.

The results have implications on the policies regarding occupational safety. First, the results suggest establishment-level information could be useful in targeting inspections, given OSHA’s limited resources on inspections. OSHA conducts around 80,000 inspections annually, which only covers less than 1% of the workplaces in the country. The inspections through the SST plan, which targeted establishments with high injury rates, are found to be effective in reducing workplace injuries. Starting from 2017, OSHA launched its Injury Tracking Application (ITA), which strengthened the requirement on injury reporting. The program requires the majority of the establishments with 250 or more employees, and establishments with 20-249 employees that are classified in certain industries with historically high injury rates to submit information on workplace injuries to OSHA, which might facilitate OSHA to targeting inspections more effectively.

Second, this study highlights the unintended effect of safety enforcement on product quality and worker productivity. While the enforcement of safety standards may contribute to the reduction of injuries and the associated costs, the increasing costs on product quality and worker productivity are largely overlooked. These unintended costs could be particularly sizable in nursing facilities. In 2013, the total expenditures for long-term care are $310 billion and the quality of care in nursing facilities is a matter of concern for residents, their families, and policy makers. Additionally, as an industry with one of the highest workplace injury rates, the working conditions nursing facilities are also extensively regulated. Since 2005, eleven states have initiated legislations on promoting safe patient handling to address the high rate of musculoskeletal injuries in health care sector (Weinmeyer, 2016), which might potentially have an unintended impact on the welfare of the patients.

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Table 1. The Starting and Closing Dates of the SST Plan, 2004-2011

Injury Rates	ODI	SST Plan	Starting Date	Closing Date
2002	2003	2004	4/19/2004	8/5/2005
2003	2004	2005	8/5/2005	6/12/2006
2004	2005	2006	6/12/2006	5/14/2007
2005	2006	2007	5/14/2007	5/19/2008
2006	2007	2008	5/19/2008	7/20/2009
2007	2008	2009	7/20/2009	10/22/2010
2008	2009	2010	10/22/2010	9/9/2011
2009	2010	2011	9/9/2011	1/4/2013

Table 2. Summary Statistics on Injury Rates, Inspections, and Operational Characteristics of Nursing Facilities

	Whole Sample	[-5, 0)	[0, 5]
Injury Case Rate			
TCR	10.683	15.679	21.250
	(7.420)	(5.339)	(5.784)
DART	6.980	11.836	16.723
	(5.236)	(1.709)	(1.685)
Inspections			
Inspections	0.044	0.032	0.387
	(0.204)	(0.175)	(0.487)
Violations	0.029	0.020	0.269
	(0.167)	(0.141)	(0.444)
Facilities			
Total Beds	120.493	122.252	117.665
	(64.263)	(60.478)	(61.263)
Total Residents	101.146	104.889	101.324
	(59.010)	(55.654)	(58.446)
In a Chain	0.492	0.543	0.563
	(0.500)	(0.498)	(0.496)
For-Profit	0.629	0.716	0.734
	(0.179)	(0.451)	(0.442)
N	13,593	2,159	788

Note: Data are matched from the OSHA Data Initiative (ODI), OSHA Integrated Management Information System (IMIS), and the Online Survey, Certification, and Reporting (OSCAR) database from the Centers for Medicare & Medicaid Services (CMS).

Table 3. The Effect of the SST Plan on Inspections, Violations, and Facility Characteristics

	Mean at SST	Local Linear
Panel A		
Inspections	0.058	0.318***
		(0.044)
Violations	0.048	0.247***
		(0.039)
Panel B		
Inspections Year Before	0.068	-0.0003
		(0.038)
Inspections Year After	0.055	0.010
		(0.028)
Panel C		
Total Bed	118.492	-5.250
		(6.673)
Total Residents	103.212	2.487
		(1.732)
In a Chain	0.543	0.073
		(0.057)
For-Profit	0.751	-0.009
		(0.050)
N		13,593

Note: The analysis covers the SST plan 2004-2009. Each cell in column 2 shows the mean of the outcome at the SST threshold. Each cell in column 3 shows an estimate from local linear models with a triangular kernel, the optimal bandwidth and robust standard errors clustered at the facility level, suggested by Calonico, Cattaneo, and Titiunik (2014). All the models include controls on the number of beds, whether in a chain, whether for profit, and state and year fixed effects.

* p<0.10, ** p<0.05, *** p<0.01

Table 4. The Effect of Inspections on Injury Case Rates One Year After the SST Plan

	Mean at SST	Reduced Form	Two-Stage
DART	14.452	-1.298*	-5.599*
		(0.821)	(2.968)
TCR	19.348	-2.061*	-7.298*
		(1.150)	(4.355)
N	4,707	4,707	4,707

Note: The analysis covers the SST plan 2004-2009 and the sample includes facilities received another survey around one year after the SST inspection cycle. DART is the number of cases involving days away from work, job restriction, or job transfer per 100 employees, and TCR is total case rate per 100 employees. Each cell in column 2 shows the mean of the outcome at the SST threshold. Each cell in column 3 and 4 shows an estimate from local linear models with a triangular kernel, the optimal bandwidth and robust standard errors clustered at the facility level, suggested by Calonico, Cattaneo, and Titiunik (2014). All the models include controls on the number of beds, whether in a chain, whether for profit, and state and year fixed effects.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 5. The Effect of Inspections on ADL Care

	Mean at SST	Reduced Form	Two-Stage
Panel A: Deficiency			
ADL Care	0.078	0.054*	0.168*
		(0.030)	(0.101)
Record	0.147	-0.017	-0.051
		(0.036)	(0.109)
Panel B: ADL Care			
Transfer	0.236	-0.016*	-0.042*
		(0.009)	(0.024)
Use Toilet	0.280	-0.032**	-0.069*
		(0.014)	(0.038)
Eat	0.143	-0.012*	-0.036*
		(0.007)	(0.018)
N		13,593	13,593

Note: The analysis covers the SST plan 2004-2009. The outcomes in Panel A are number of deficiency citations on each standard. The outcomes in Panel B are the fraction of residents receiving full assistance on ADLs. Each cell in column 2 shows the mean of the outcome at the SST threshold. Each cell in column 3 and 4 shows an estimate from local linear models with a triangular kernel, the optimal bandwidth and robust standard errors clustered at the facility level, suggested by Calonico, Cattaneo, and Titiunik (2014). All the models include controls on the number of beds, whether in a chain, whether for profit, and state and year fixed effects.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 6. The Effect of Inspections on Resident Health Outcomes

	Mean at SST	Reduced Form	Two-Stage
Contracture	0.280	0.007	0.016
		(0.024)	(0.061)
Pressure Sores	0.067	-0.008	0.002
		(0.005)	(0.010)
Weight Change	0.077	-0.001	0.005
		(0.007)	(0.018)
Behavior	0.272	0.036*	0.085**
		(0.021)	(0.043)
N		13,593	13,593

Note: The analysis covers the SST plan 2004-2009. Each cell in column 2 shows the mean of the outcome at the SST threshold. Each cell in column 3 and 4 shows an estimate from local linear models with a triangular kernel, the optimal bandwidth and robust standard errors clustered at the facility level, suggested by Calonico, Cattaneo, and Titiunik (2014). All the models include controls on the number of beds, whether in a chain, whether for profit, and state and year fixed effects.
* p<0.10, ** p<0.05, *** p<0.01

Table 7. The Effect of Inspections on Deficiencies and Source of Payment

	Mean at SST	Reduced Form	Two-Stage
Deficiencies on Transfer and Discharge	0.031	-0.002	-0.017
		(0.022)	(0.057)
Share of Medicaid Residents	0.641	-0.012	-0.052
		(0.018)	(0.047)
N			13,593

Note: The analysis covers the SST plan 2004-2009. Each cell in column 2 shows the mean of the outcome at the SST threshold. Each cell in column 3 and 4 shows an estimate from local linear models with a triangular kernel, the optimal bandwidth and robust standard errors clustered at the facility level, suggested by Calonico, Cattaneo, and Titiunik (2014). All the models include controls on the number of beds, whether in a chain, whether for profit, and state and year fixed effects.

* p<0.10, ** p<0.05, *** p<0.01

Table 8. The Effect of Inspections on Nursing Hours per Patient Day

	Mean at SST	Reduced Form	Two-Stage
Nursing Aides	3.459	0.024	0.106
		(0.091)	(0.267)
Licensed Practical Nurses	1.208	-0.037	-0.059
		(0.047)	(0.112)
Registered Nurses	0.547	0.019	0.065
		(0.036)	(0.105)
Nurses with Administrative Duties	0.278	0.024	0.077*
		(0.017)	(0.046)
N		13,593	13,593

Note: The analysis covers the SST plan 2004-2009. Each cell in column 2 shows the mean of the outcome at the SST threshold. Each cell in column 3 and 4 shows an estimate from local linear models with a triangular kernel, the optimal bandwidth and robust standard errors clustered at the facility level, suggested by Calonico, Cattaneo, and Titiunik (2014). All the models include controls on the number of beds, the number of residents, whether in a chain, whether for profit, and state and year fixed effects.
* p<0.10, ** p<0.05, *** p<0.01

Table 9. The Effect of Inspections on Quality of Care in Nursing Facilities, Robustness Tests

	Pre-SST Outcomes 2008-2010	Post-SST Outcomes 2008-2010	State Plan
Panel A. ADL Care			
Deficiency	-0.038	0.069*	-0.012
	(0.082)	(0.041)	(0.056)
Transfer	-0.024	-0.041**	-0.0001
	(0.019)	(0.016)	(0.021)
Toilet	0.001	-0.043**	0.010
	(0.023)	(0.019)	(0.025)
Eating	-0.009	-0.018*	0.006
	(0.015)	(0.011)	(0.016)
Panel B. Health Outcomes			
Contracture	-0.014	0.006	0.033
	(0.037)	(0.030)	(0.029)
Pressure Sores	-0.004	-0.002	0.008
	(0.013)	(0.005)	(0.008)
Weight Change	-0.019	0.002	0.022
	(0.012)	(0.009)	(0.014)
Behavior	0.009	0.030	-0.011
	(0.037)	(0.020)	(0.033)
N	6,171	6,171	3,509

Note: Each cell shows a reduced form estimate from a different regression for the given outcome (rows). The estimates are from local linear models with a triangular kernel, the optimal bandwidth and robust standard errors clustered at the facility level, suggested by Calonico, Cattaneo, and Titiunik (2014). All the models include controls on the number of beds, whether in a chain, whether for profit, and state and year fixed effects.
* p<0.10, ** p<0.05, *** p<0.01

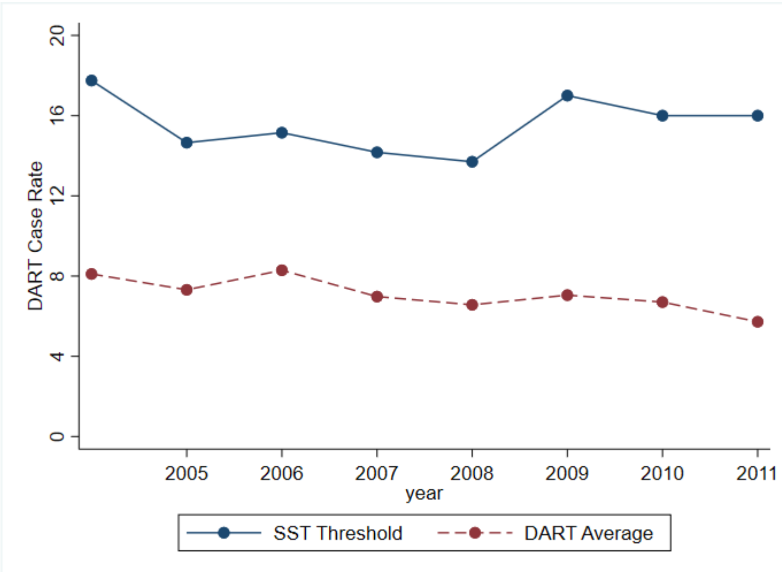


Figure 1. Days Away, Restricted, and Transfer (DART) Case Rate Threshold of Site-Specific Targeting (SST) Plan and Average DART Case Rate, Nursing Facilities 2004-2011

Notes: DART case rate is calculated as (number of cases involving days away from work, job transfers or restrictions * 200,000) / total employee hours worked, which gives the case rate per 100 full time equivalent employees.

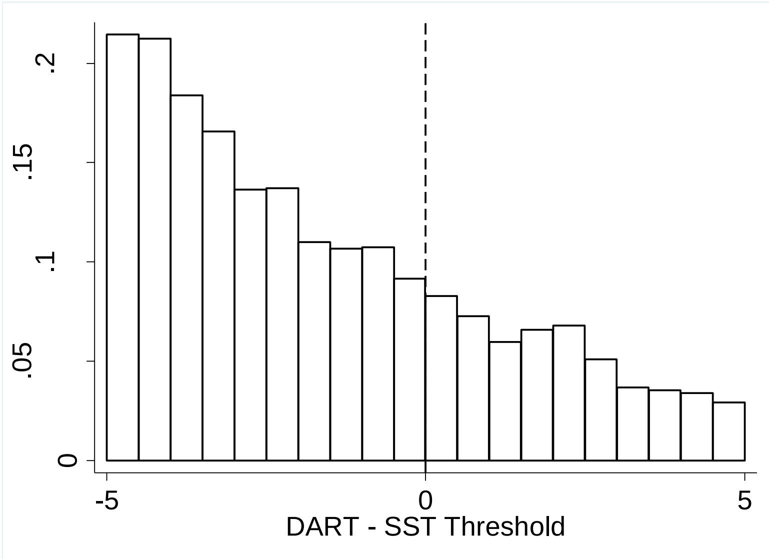


Figure 2. Distribution of Nursing Facilities by DART Case Rate Relative to the SST Threshold

Note: N=2,947. McCrary's density test shows the difference of density at the threshold is small and insignificant (log density = 0.026, SE = 0.092).

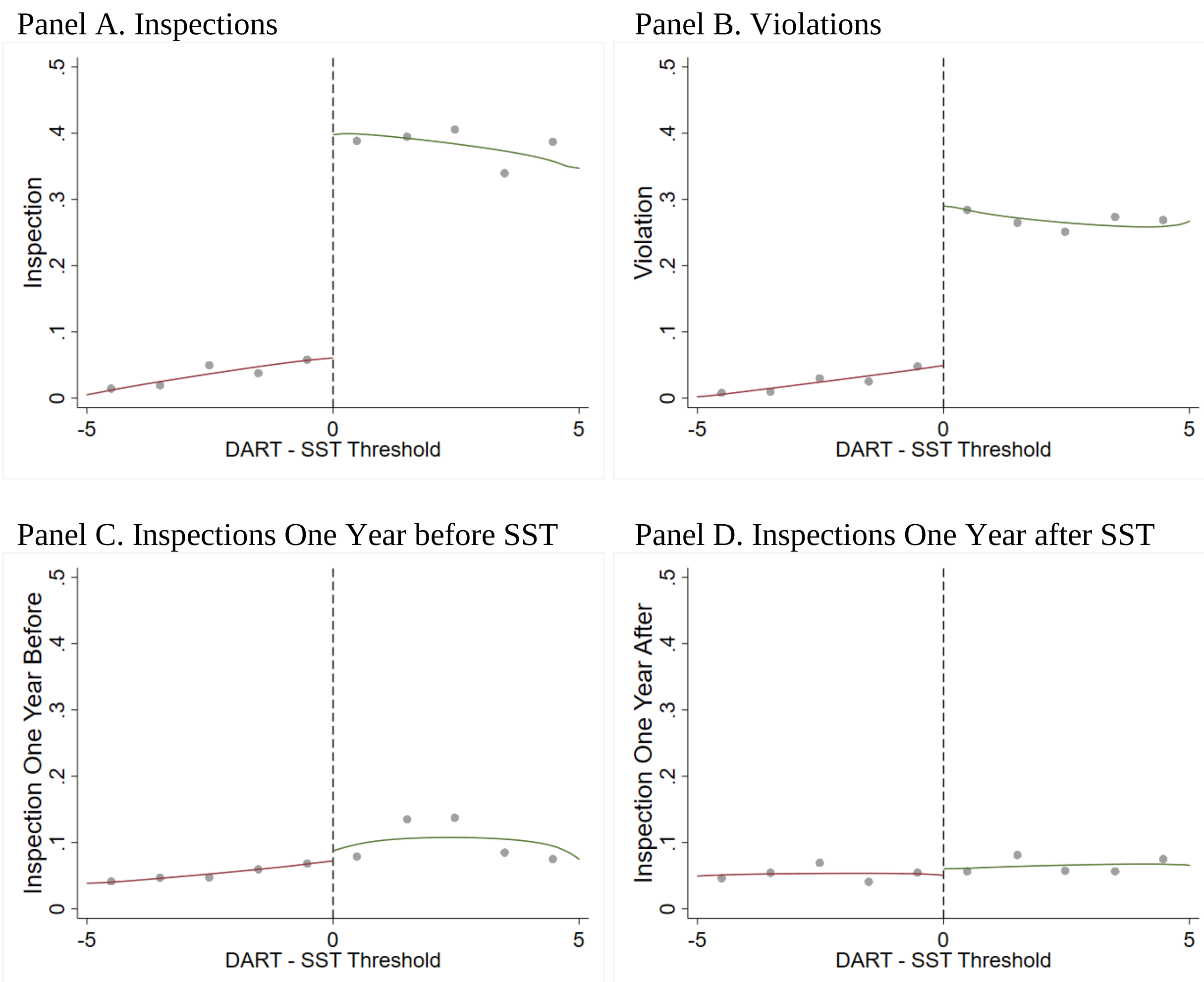
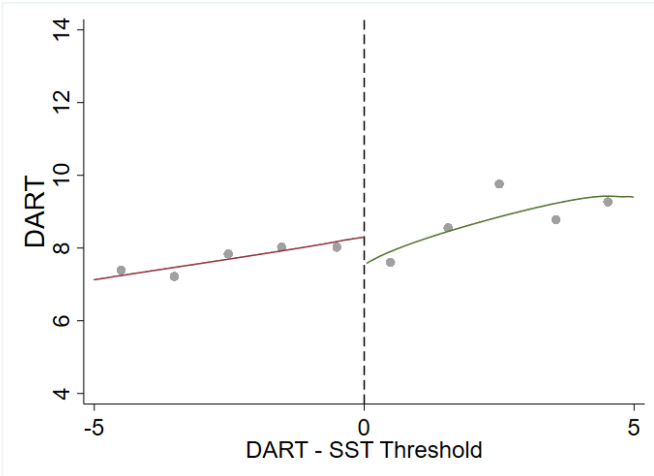


Figure 3. Frequency of Inspections and Violations by DART Case Rate Relative to the SST Threshold

Notes: The graphs show the frequency of inspections and violations by (DART – SST threshold). The lines are fitted values from local linear regressions. N=2,947.

Panel A. DART Case Rate One Year after SST



Panel B. TCR One Year after SST

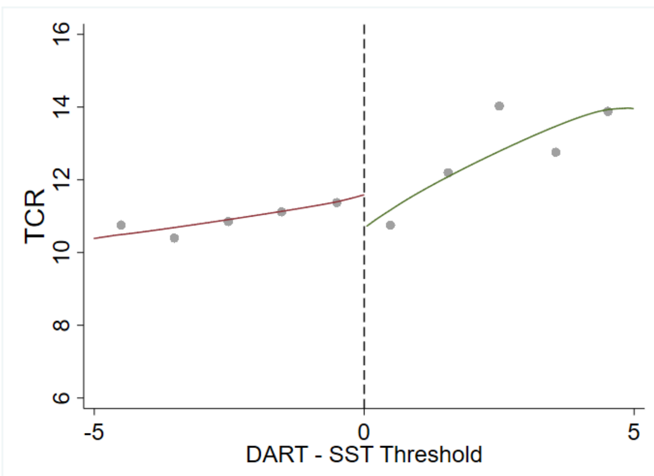
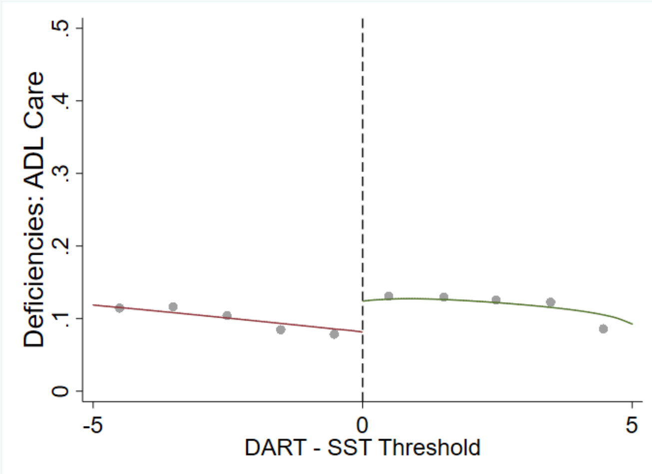


Figure 4. The Effect of the SST Plan on the Injury Case Rates One Year After

Note: The sample includes nursing facilities with another ODI survey fours year after the initial survey. The outcomes represent injury rates around one year after the SST plan. DART is the number of cases involving days away from work, job restriction, or job transfer per 100 employees, and TCR is total case rate per 100 employees. The lines are fitted values from local linear regressions. N=1,328.

Panel A. Deficiencies: ADL Care



Panel B. Deficiencies: Record-Keeping

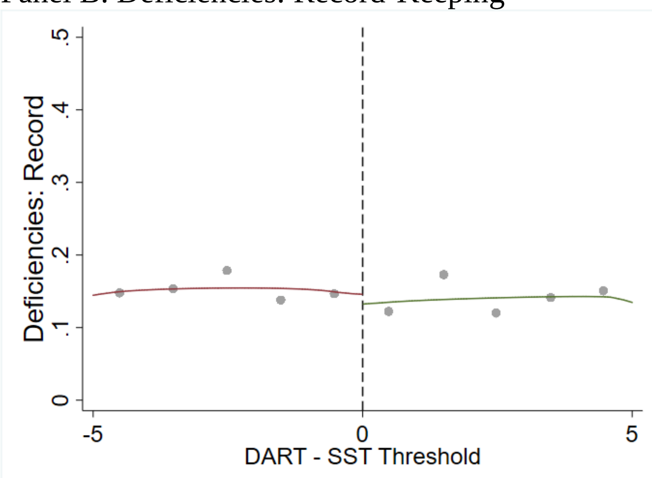


Figure 5. The Effect of the SST Plan on the Deficiencies of Nursing Facilities

Note: The outcomes are number of deficiency citations in each category, measured around one year after the SST plan. The lines are fitted values from local linear regressions. N=2,947.

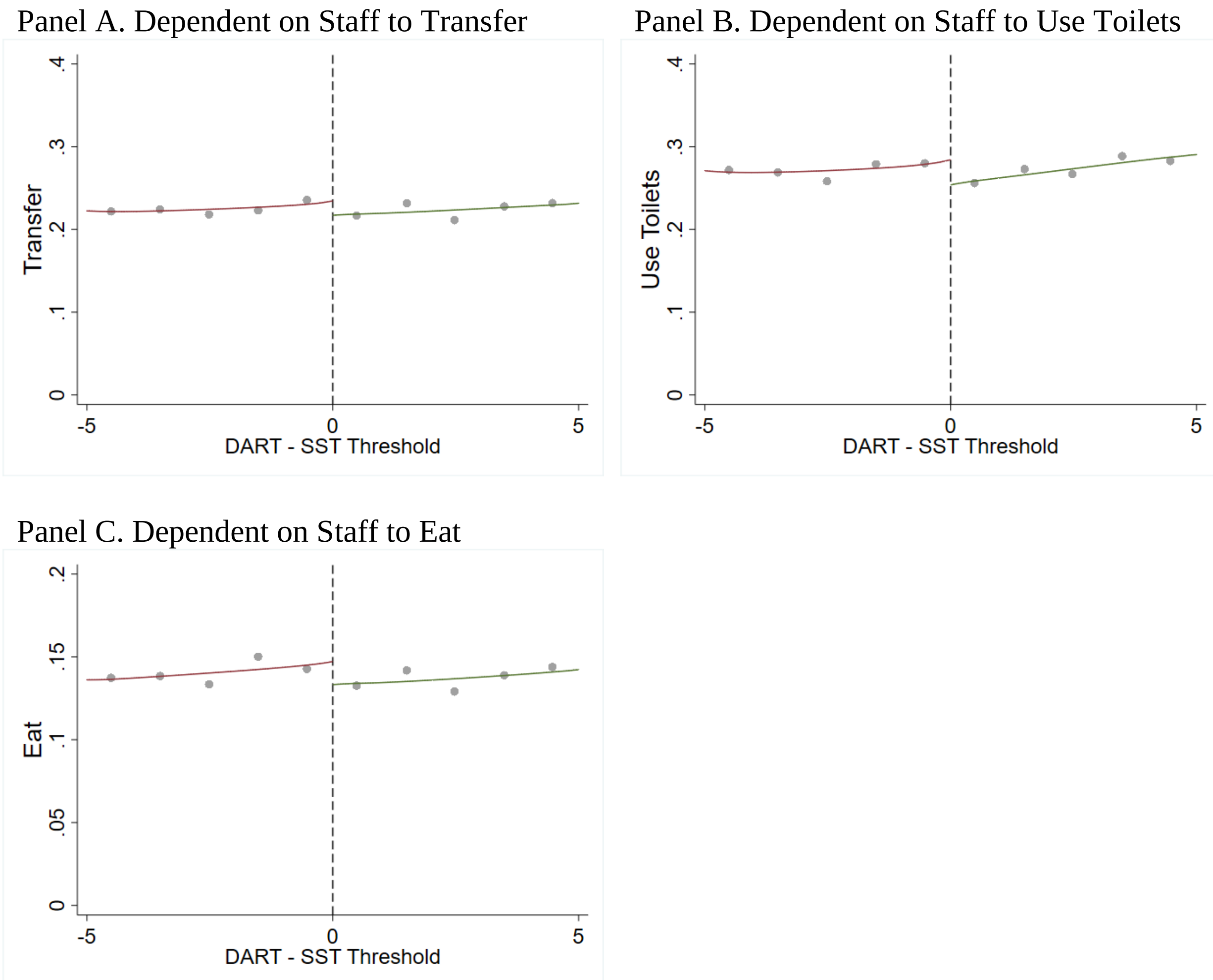


Figure 6. The Effect of the SST Plan on the Fraction of Residents Receiving Full Assistance from Staff with Activities of Daily Living (ADLs)

Note: Outcomes are measured around one year after the SST plan. The dependency of ADLs is measured as the fraction of residents fully dependent on staff with ADLs. The lines are fitted values from local linear regressions. N=2,947.

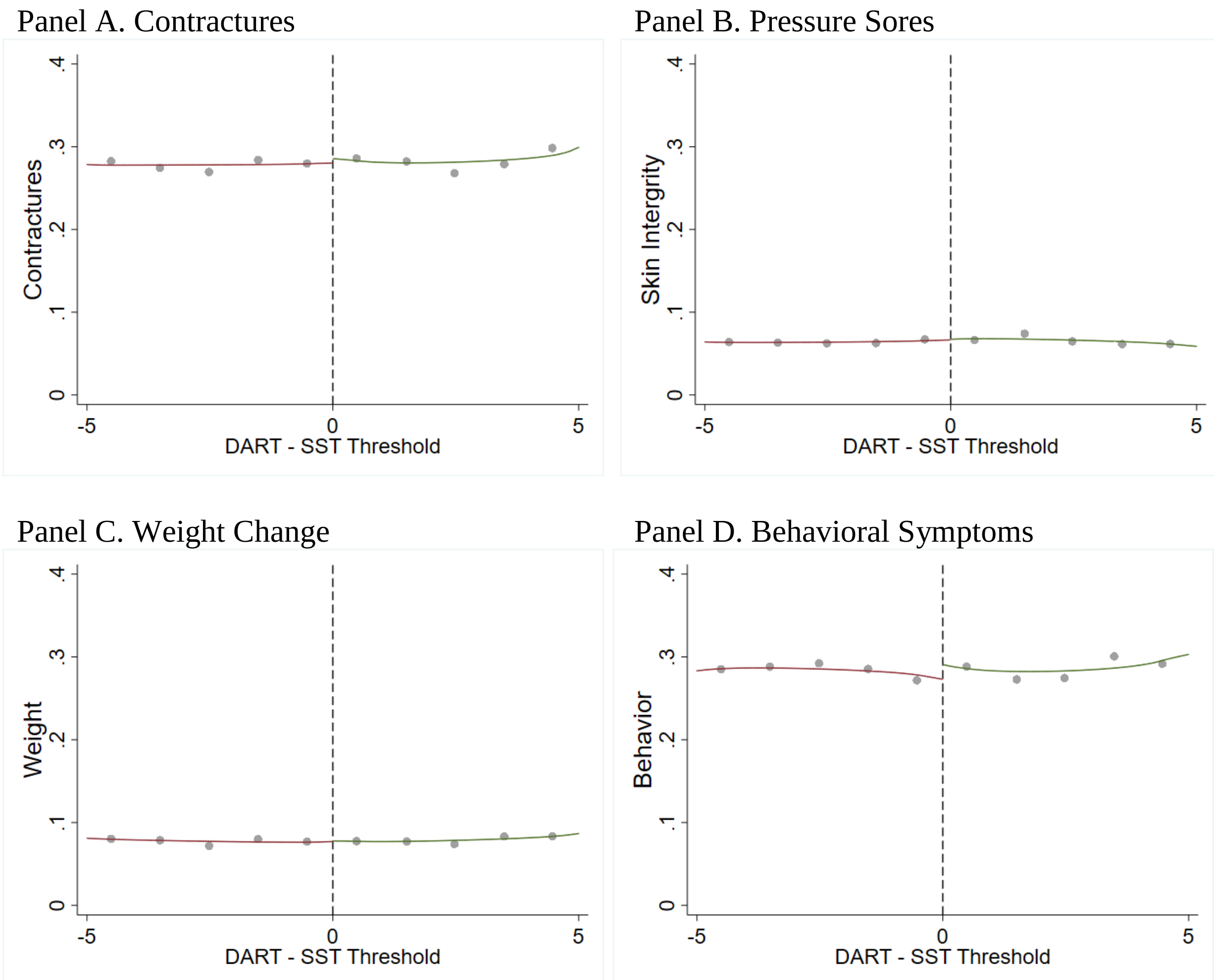
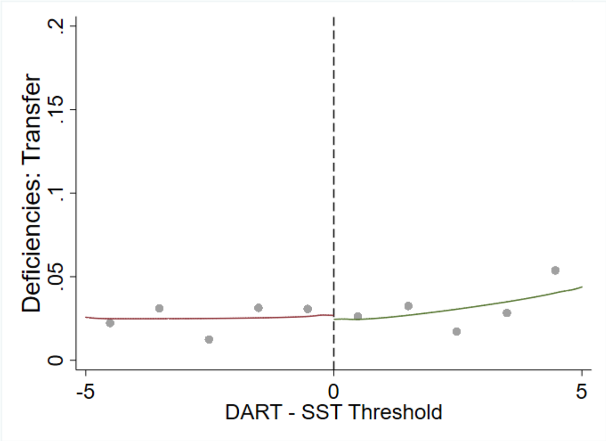


Figure 7. The Effect of the SST Plan on Resident Health Outcomes

Note: The health outcomes are measured as percent of residents with specific conditions, around one year after the SST plan. The lines are fitted values from local linear regressions. N=2,947.

Panel A. Deficiencies: Transfer and Discharge



Panel B. Share of Medicaid Residents

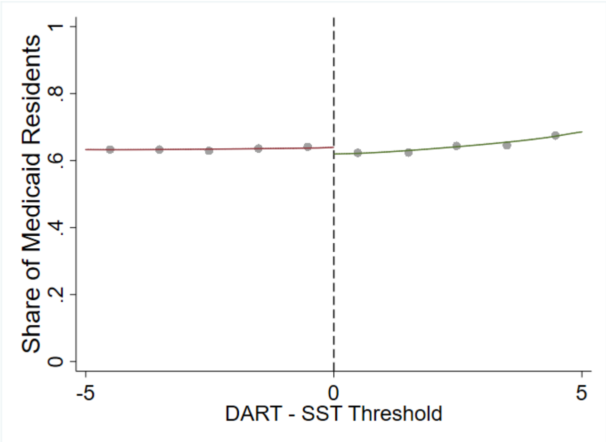
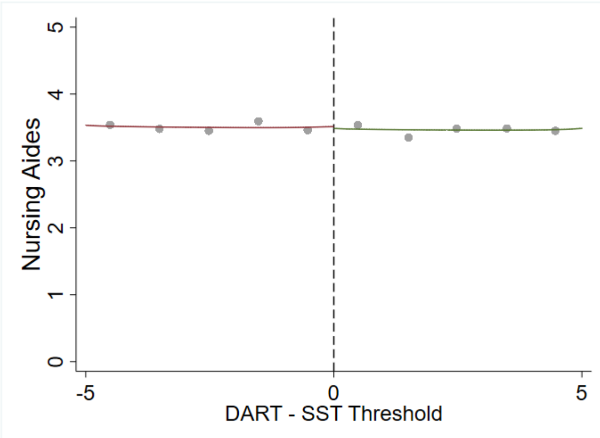


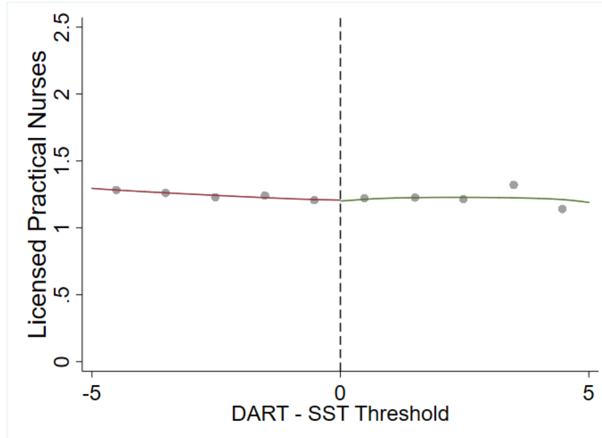
Figure 8. The Effect of the SST Plan on the Number of Deficiencies and Source of Payment

Note: The outcomes are measured around one year after the SST plan. The lines are fitted values from local linear regressions. N=2,947.

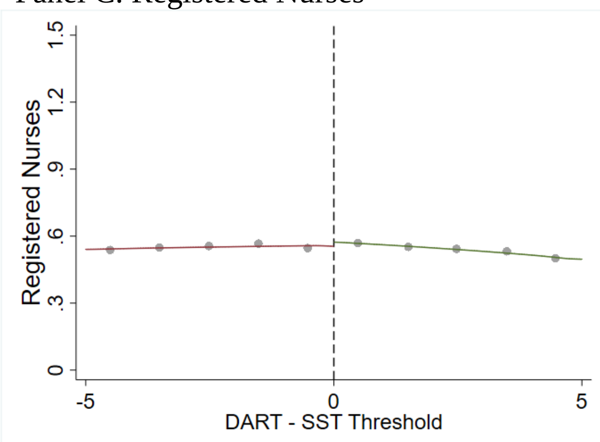
Panel A. Aides



Panel B. Licensed Practical Nurses



Panel C. Registered Nurses



Panel D. Nurses with Administrative Duties

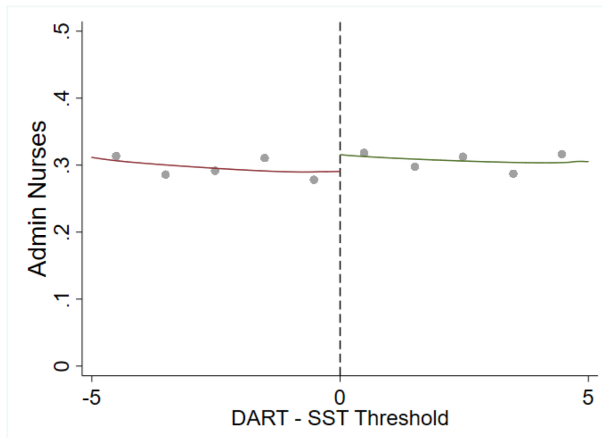


Figure 9. The Effect of the SST Plan on the Nursing Hours of Nursing Facilities

Note: Staffing level is measured as nursing hours per patient day, around one year after the SST plan. The lines are fitted values from local linear regressions. N=2,947.

Chapter 2. The Effect of Workplace Inspections on Worker Safety

1. Introduction

In 2007, the estimated cost of on-the-job injuries in the US was $192 billion (Leigh 2011). While employers may independently invest in workplace safety, investment may be suboptimal if employers do not internalize the full costs of worker injuries. To attempt to achieve the social optimum, governments could enforce safety and health regulations through workplace inspections, the primary responsibility of the US Occupational Safety and Health Administration (OSHA). However, this approach depends on whether regulations and workplace inspections are effective. This is difficult to determine empirically since inspections are generally targeted at high-risk establishments (Kniesner and Leeth 2014; Smith 1979). As a result, inspections and worker safety would be negatively correlated, which would confound any positive, causal effect of the former on the latter.

In this study, we attempt to identify the causal effect of inspections on worker safety. The identification strategy exploits quasi-experimental variation in inspections generated by OSHA's Site Specific Targeting (SST) plan. The SST plan, implemented in 1999, targeted establishments with high rates of accidents and injuries for inspection. The plan used data from the OSHA Data Initiative (ODI), which collected establishment-level data on accidents and injuries directly from employers. Using these data, the plan prioritized establishments for inspection using case-rate cutoffs. One set of cutoffs defined the primary inspection list, and a lower set of cutoffs defined the secondary inspection list. This process generated a discontinuous increase in inspections at the cutoff, particularly for the primary inspection list. Using the fuzzy regression discontinuity (FRD) design, the discontinuity in inspections is used to identify the causal effect of inspections on worker safety.

Data on accidents and injuries come from the ODI, conducted annually from 1996 to 2011. These data are used to predict inspections during the SST plan and to measure worker safety outcomes after the SST plan. The data report the rate of cases involving days away from work, job restrictions, and job transfers (DART). To determine whether an establishment is inspected during the SST plan, the ODI data are matched to OSHA's Integrated Management Information System (IMIS), which contains data on all establishments inspected during the analysis period.

We first estimate the discontinuous increase in inspection outcomes, particularly at the DART rate cutoff for the primary inspection list. Using local linear regression, the cutoff is associated with a 22.7 percentage point increase in inspections related to the SST plan, a 17.5 percentage point increase in any citations, and a 15.4 percentage point increase in any penalty. The cutoff is not associated with a change in "unprogrammed" inspections, which are unrelated to the SST plan.

We then estimate the effect of an inspection on worker safety. Using the FRD design and local linear regression, the average effect of an inspection on the DART rate is -1.792 per 100 full-time equivalent workers – a reduction of 20 percent relative to the post-inspection DART rate near the cutoff. Moreover, the effect on the DART rate is most evident for manufacturing establishments, particularly below the 90^{th} percentile of the DART distribution post-inspection. Treatment effects are less evident for other case rates and for other industries. Given the empirical strategy, the treatment effect estimates pertain only to establishments near the cutoff, and thus are not generalizable to establishments away from the cutoff.

Because case-rates are self-reported, a valid concern is that employers may underreport their case rate to avoid inspection (Ruser and Smith 1988). If the tendency to underreport is greater among recently inspected employers under the SST plan, underreporting could account for the results of this study. This may not be the case for four reasons. First, under the SST plan, employers report their case rates before the SST cutoffs are determined, which limits the ability of employers to avoid inspection entirely. Second, the case rate distribution is smooth at the cutoff, suggesting that employers did not underreport case rates to avoid inspection, at least not locally. Third, OSHA inspections include an audit of previously-recorded case rates, which may deter employers from underreporting.[20] Finally, citations for record-keeping violations were extremely rare among establishments that were inspected again within one year after the SST inspection cycle. Among establishments above the cutoff under the SST plan, only 0.32 percent were cited for a record-keeping violation during a subsequent inspection. Below the cutoff, only 0.35 percent were cited.

Regarding efficiency, an important question is whether the gains from the additional inspections exceed the marginal costs. The gains include the statistical value of averted injuries as well as the fiscal externalities through, for example, social insurance programs.[21] The costs include both the cost of the inspection to OSHA and the cost of compliance to employers.[22] To improve efficiency, OSHA should target establishments for inspection in which the effect on worker safety is greatest. In this study, the effect is most evident in manufacturing and less evident in health services, the largest two-digit industries represented in the ODI data.

[20] Kniesner and Leeth (2014) note that deterrence effects are limited by the low likelihood of inspection and relatively small financial penalties.
[21] For a review of estimates on the value of statistical injury, see Viscusi and Aldy (2003).
[22] For a discussion on compliance costs, see Kniesner and Leeth (2014).

2. Background

5.1. Occupational Safety and Health Administration

The goal of the Occupational Safety and Health Act, passed by the US Congress in 1970, is "to assure safe and healthful working conditions for working men and women." To achieve this goal, the Occupational Safety and Health Administration (OSHA) was created to codify and enforce safety and health regulations. Regulations include specification standards, such as safety guards for machinery or equipment, and performance standards, such as limits on exposure to hazardous chemicals (Kniesner and Leeth 2014). To enforce regulations, OSHA educates employers and employees, inspects worksites for workplace hazards, and levies financial penalties on employers for serious or repeated violations.

OSHA inspections are either programmed or unprogrammed. Unprogrammed inspections result from fatal or catastrophic accidents, employee complaints, or referrals from non-employees, whereas programmed inspections are intended to identify and abate workplace hazards before an accident or illness occurs. In fiscal year 2015, OSHA conducted 16,527 programmed inspections and 19,293 unprogrammed inspections.[23] Among unprogrammed inspections, 912 were due to fatal or catastrophic accidents, 9,037 were due to employee complaints, 4,705 were due to referrals, and 4,639 were due to other reasons.

5.2. OSHA Inspections and Worker Safety

The literature on OSHA inspections and worker safety finds a wide range of effects, depending on the identification strategy, analysis period, firm size, definition of treatment

[23] These figures exclude State Plan inspections, which are conducted by states under the purview of OSHA. In fiscal year 2016, State Plan inspections totaled 43,105.

(inspection versus citation or penalty), and worker safety outcome (overall injuries versus specific types).[24] For identification, some studies exploit the timing of an inspection, arguing that establishments inspected earlier in the year have more time to remediate workplace hazards (McCaffrey 1983; Ruser and Smith 1991; Smith 1979). These studies find no effect of inspections on case rates, except for a small decrease in 1973 (Smith 1979). These estimates may be downward biased, however, since establishments inspected earlier have higher rates of accidents (Gray and Scholz 1993). Cooke and Gautschi (1981) relate changes in case rates to the number of citations issued during an inspection. They find that citations decrease days lost from injury by 23 percent in plants with more than 200 workers. However, the relatively large effect may be attributable to mean reversion, whereby a high case rate in one period, which may precipitate an inspection, is followed by a lower rate the next period (Ruser 1995). A study by Levine, Toffel, and Johnson (2012) uses experimental data from California in 1996 to 2006. By exploiting random assignment of an inspection among 409 establishments, they find that inspections reduce injuries by 9.4 percent, with no detectable effect on employment, sales, or firm survival. A limitation of their study is that it is restricted to high-risk industries in California and therefore is not generalizable to other industries or states (Kniesner and Leeth 2014). Finally, a report by Summit Consulting (Peto et al. 2016) uses the same identification strategy as this study, but only uses ODI data collected in 2007. They find a small and statistically insignificant effect of an inspection on worker safety.[25]

[24] Kniesner and Leeth (2014) provide a review of the literature. Some studies differentiate inspections by whether they result in a citation or penalty, arguing that only these inspections should affect worker safety (Cooke and Gautschi. 1981; Mendeloff and Gray 2005; Gray and Scholz 1993; Haviland et al. 2010).

[25] When we limit our sample specifically to 2007, we also find small and statistically insignificant effects.

5.3. Site Specific Targeting Plan

In 1999, OSHA drastically changed its procedure for targeting programmed inspections. Before 1999, programmed inspections were targeted at industries with high rates of accidents and injuries. This was accomplished using industry-level data on accidents and injuries collected by the Bureau of Labor Statistics. However, after an inspection, many establishments in high-risk industries were found to be relatively safe, revealing a high degree of within-industry variation in worker safety. Thus, targeting high-risk industries seemed to be relatively inefficient at targeting high-risk establishments.

To better target high-risk establishments, OSHA created the ODI in 1996. The goal of the ODI was to collect data on accidents and injuries at the establishment level. OSHA requires most establishments with 10 or more full-time employees to record accidents and injuries using OSHA's Form 300, provided in the Appendix. Per the form, employers record cases involving four outcomes: (1) death, (2) days away from work, (3) job restrictions or transfers, or (4) medical attention beyond first aid. Each year, the ODI collected Form 300 data for the previous calendar year from a sample of establishments meeting the sampling criteria.[26] The ODI data contains several case rates, calculated annually per 100 full-time equivalent workers.[27] The total case rate (TCR) includes all four cases listed above. A second rate includes only days away, job

[26] The sample was chosen from Dun & Bradstreet data, a comprehensive registry of businesses in the US. The sampling criteria are described in the Data section.
[27] To calculate rates, the ODI asks employers to report the number of employees and the total hours worked by employees during the previous calendar year. This information is not reported in the ODI data.

restrictions, and job transfers (DART).[28] A third case rate includes only days away from work (DAFWII). The ODI does not report rates for the cases separately.

The ODI data were then used to implement OSHA's SST plan. To target high-rate establishments, the SST plan prioritized establishments using case-rate cutoffs. For example, the ODI in 2003 collected case-rate data for calendar year 2002, and the SST plan used these data to target programmed inspections from April 2004 to August 2005. (The inspection calendar for other SST cycles are provided in the Appendix Table.) The primary inspection list included establishments with a DART rate greater than 15 or a DAFWII rate greater than 10. A secondary inspection list included establishments with a DART rate greater than 10 or a DAFWII rate greater than 4. Additionally, all establishments with a DART rate greater than 7 were mailed a letter stating that their DART rate was high relative to the national average. Occasionally, the case-rate cutoffs changed, reflecting changes in the case-rate distribution and OSHA's resources to conduct inspections.

While all establishments on the primary list were targeted for an inspection, not all establishments were inspected, and those that were inspected did not always have the highest case rates (US Department of Labor 2012).[29] The low inspection rate was attributed, in part, to limited resources (US Department of Labor 2012). To address this limitation, each of the 81 OSHA Area Offices determined the number of establishments it could reasonably inspect, and then randomly selected a subset of establishments for inspection. However, treatment assignment did not perfectly predict inspection outcomes (Johnson, Levine, and Toffel 2017).

[28] In 2002, the DART replaced a rate that includes only cases involving lost work days due to injury or illness (LWDII), though the DART and the LWDII are nearly identical.
[29] For example, from August 2010 through September 2011, only 16 percent of establishments on the primary and secondary inspection list were ultimately inspected.

Importantly, the ODI data were collected in 45 US states and the District of Columbia, but the SST plan was implemented in only 35 states. This includes 29 states that are covered directly by OSHA and 6 states that are covered by state-level agencies – known as State Plans – approved by OSHA.

3. Methodology

The empirical objective is to identify the causal effect of an OSHA inspection on worker safety. The effect is identified using quasi-experimental variation in inspections generated by OSHA's SST plan. Specifically, the SST established a case-rate cutoff, and establishments exceeding the cutoff were targeted for a programmed inspection. If this process generated a discontinuous increase in inspections at the cutoff, and if establishments just above and below the cutoff are similar, then the increase in inspections at the cutoff may be used to identify the causal effect of an inspection on worker safety.

5.1. Fuzzy Regression Discontinuity

According to the potential outcomes framework (Rubin 1974; Holland 1986), each establishment has two potential outcomes: worker safety without an inspection, denoted $Y_i(0)$, and worker safety with an inspection, denoted $Y_i(1)$. For each establishment, the causal effect of an OSHA inspection is defined as $\tau_i = Y_i(1) - Y_i(0)$. The fundamental problem for identifying τ_i is that only one outcome – either $Y_i(1)$ or $Y_i(0)$ – is observed for each establishment.

To plausibly identify causal effects, the empirical strategy utilizes the fuzzy regression discontinuity (FRD) design (Hahn, Todd, and Van der Klaauw 2001; Imbens and Lemieux 2008). The FRD design requires three main assumptions. First, whether an establishment is inspected, denoted by the variable D_i, must be partially determined by whether a running variable X_i exceeds a cutoff c:

(1) $$\lim_{x \uparrow c} E[D_i|X_i = x] < \lim_{x \downarrow c} E[D_i|X_i = x].$$

In this case, the likelihood of treatment increases at the cutoff c. Second, the increase in the likelihood of an inspection is due only to compliers, defined as those who are treated just above the cutoff, but would not have been treated in the absence of the SST plan (Imbens and Lemieux 2008). Third, the conditional mean functions $E[Y(0)|X_i = x]$ and $E[Y(1)|X_i = x]$ are continuous at the cutoff with respect to the running variable X_i. If so, $\lim_{x \uparrow c} E[Y|X = x]$ represents the counterfactual of $\lim_{x \downarrow c} E[Y|X = x]$ in the absence of the SST plan.

With these assumptions, the FRD estimand is given by:

(2) $$\tau_{FRD} = \frac{\lim_{x \downarrow c} E[Y|X = x] - \lim_{x \uparrow c} E[Y|X = x]}{\lim_{x \downarrow c} E[D|X = x] - \lim_{x \uparrow c} E[D|X = x]}.$$

The numerator measures the difference in the mean outcome Y above and below the cutoff, and the denominator measures the difference in the treatment D above and below the cutoff. By dividing the former by the latter, the FRD estimand measures the average effect of treatment among compliers.

5.2. Distributional Effects

The FRD estimand measures the average treatment effect among compliers. However, the effect among compliers may differ across the distribution of the outcome variable Y. On one hand, establishments with high Y, which are presumably more dangerous, have greater scope for remediating workplace hazards. On the other hand, these establishments may face greater idiosyncratic risk beyond the purview of OSHA regulations and enforcement. Thus, the effect of an inspection across the distribution of the outcome variable Y is ambiguous.

To estimate distributional effects, the cumulative density function (CDF) for Y is estimated among compliers just above the cutoff, where they are treated, and among counterfactual compliers just below the cutoff, where they are not treated. The estimands for the conditional CDFs are provided by Frandsen, Frolich, and Melly (2012). Above the cutoff, the conditional CDF is given by:

(3) $$F_{Y(1)|\Omega}(y) = \frac{\lim_{x \downarrow c} E[1(Y \leq y)D|X = x] - \lim_{x \uparrow c} E[1(Y \leq y)D|X = x]}{\lim_{x \downarrow c} E[D|X = x] - \lim_{x \uparrow c} E[D|X = x]}.$$

Below the cutoff, the conditional CDF is given by:

(4) $$F_{Y(0)|\Omega}(y) = \frac{\lim_{x \downarrow c} E[1(Y \leq y)(1 - D)|X = x] - \lim_{x \uparrow c} E[1(Y \leq y)(1 - D)|X = x]}{\lim_{x \downarrow c} E[1 - D|X = x] - \lim_{x \uparrow c} E[1 - D|X = x]}.$$

Both CDFs are conditional on compliers, denoted by Ω. At each value of $Y = y$, the distributional impact of treatment among compliers is measured by $F_{Y(1)|\Omega}(y) - F_{Y(0)|\Omega}(y)$.

5.3. Estimation

Treatment effects are estimated using nonparametric, local linear regression. An advantage of local linear regression is that observations can be weighted more near the cutoff where the estimands are evaluated (Imbens and Lemieux 2008). For example, the term $\lim_{x \downarrow c} E[Y|X = x]$ is estimated by solving

(5) $$\min_{\alpha_{YR}, \beta_{YR}} \sum_{c \leq X_i \leq c + h_{YR}} \left(Y_i - \alpha_{YR} - \beta_{YR}(X_i - c)\right)^2 \mathrm{K}\left(\frac{X_i - c}{h_{YR}}\right).$$

The term $X_i - c$ is the distance of observation i to the cutoff c, among establishments with X_i betweeb c and $c + h_{YR}$, so that α_{YR} corresponds to $\lim_{x \downarrow c} E[Y|X = x]$. The parameters are estimated by minimizing the sum of the squared deviations, weighted by the kernel function $\mathrm{K}\left(\frac{X_i - c}{h}\right)$. Estimation is accomplished using a procedure developed by Calonico, Cattaneo, and

Titiunik (2014), which estimates the optimal bandwidth h and provides a robust, bias-correction for $\hat{\tau}_{FRD}$. The standard errors are clustered by establishment. The kernel function $\mathrm{K}(.)$ is triangular.

5.4. Data

Data for the running variable X_i and the outcome variable Y_i come from OSHA's ODI. Stated above, the ODI collected Form 300 data from a sample of establishments each year. The sample was selected from Dun & Bradstreet data, a registry of establishments in the US. In general, the ODI targeted establishments with a minimum number of employees in manufacturing and other industries with injury rates above the national average, excluding construction. While the goal of the ODI was to survey all establishments meeting the target criteria at least once every three years (Johnson, Levine, and Toffel 2017), the sampling criteria often changed. For example, dairy farms were covered in 1998, but not in 2000; and ornamental nurseries were covered in 2000, but not in 1998. Also, before 1999, the sample excluded establishments with fewer than 40 employees, but the cutoff was increased to 60 employees starting in 1999.

From 1996 to 2011, the ODI surveyed approximately 60,000 to 80,000 establishments each year. Regarding accidents and injuries, the data report the TCR, the DART, and the DAFWII, though the DAFWII is only available for calendar years 2002 and beyond. Case rates are measured per 100 full-time equivalent workers.

To construct the analysis sample, the ODI data were first pooled across years 1996 to 2011, yielding 1,018,600 establishment-by-year observations. Observations were dropped if they appear to be a duplicate record or if the establishment's name and address are missing, eliminating 0.46 percent of the sample. The observations were then stacked by establishment based on the establishment's name and address, yielding 341,302 unique establishments, of which 188,178 have more than one observation.[30] Firms may not be observed in subsequent years due to establishment closure, change of address, or a change to the ODI sampling criteria.

The data were then limited to pairs of observations spaced four calendar years apart. This yields 252,382 paired observations in which the first year occurs in 1996 to 2007.[31] The first observation is used for the running variable X_i, and second observation is used for the outcome variable Y_i. The lag of four years was chosen so that the second observation corresponds to the first calendar year after the SST plan. For example, the data for 2002, collected in 2003, were used to target programmed inspections from April 2004 to August 2005. Thus, the outcome variable Y_i is measured in 2006.

To derive the analysis sample of interest, three additional restrictions are imposed. First, observation pairs are dropped if the first year occurs in 1996, as these data were not used to implement the SST plan.[32] Second, the sample is restricted to states that participated in the SST plan, which includes all 29 states under federal jurisdiction with respect to OSHA and six states that operate state plans. Third, observations pairs are excluded if the case rate from the ODI is missing or exceeds 100, eliminating 1.9 percent of the sample.[33] The remaining sample contains 154,808 paired observations among 61,702 unique establishments, for an average of 2.5 paired observations per establishment. 25,460 establishments have only one observation pair.

[30] Establishment name and address were standardized before linking. See Appendix for more details.
[31] Observations in 2008 and after have no second observations since there is no available data after 2011.
[32] We use the data for 1996 to conduct a placebo test after presenting the baseline results.
[33] This restriction eliminates extreme outliers, but has no impact on the results.

The cutoff c was identified from administrative reports from the SST plan. The cutoffs varied by inspection list (primary versus secondary), case type, industry, and SST cycle (see Appendix Table).

To measure the inspection indicator D_i, the ODI data are merged to OSHA's IMIS. The IMIS contains information on over three million OSHA inspections from 1972 to September 2016, at the time the data were downloaded.[34] For each inspection, the data report the type of inspection, programmed or unprogrammed; the citations recorded during the inspection; and the penalties levied for each citation, if any. The inspection indicator D_i is measured only during the SST plan cycle. Thus, in the example above, D_i equals one if an establishment matches to a programmed inspection record in the IMIS from April 2004 to August 2005 and zero otherwise.

The ODI data were merged to the IMIS based on the name and address of the establishment, including the street number, street name, city, state, and zip code.[35] Although the data were cleaned and standardized before matching, there may be both false-negatives and false-positives in matching. A false-negative occurs if an establishment had been inspected during the SST cycle, but did not match to its inspection record in the IMIS. Conversely, a false-positive occurs if an establishment had not been inspected during the SST cycle, but matched to an inspection record in the IMIS. We assume false-positives are rare given the stringency of the matching criteria, described in the Appendix.

The remaining concern for false-negatives is that the estimate of τ_{FRD} may be biased. For example, if the inspection rate is biased downward by a proportional factor $0 < \pi < 1$, meaning only π percent of inspected establishments successfully match to the IMIS, then the

[34] The IMIS data are updated daily and are subject to revision. For this project, the data were downloaded in December 2017.
[35] Additional details of the merging procedure are provided in the Appendix.

estimate of τ_{FRD} would be biased upwards in absolute value by a factor of $1/\pi$. This can be seen in equation (2), with both $\lim_{x \downarrow c} E[D|X = x]$ and $\lim_{x \uparrow c} E[D|X = x]$ factored by π. Our best estimate of π is 82.7 percent, which is the match rate to the IMIS among establishments that, according to administrative records, are known to have been inspected under the SST plan.[36] Thus, the estimate of τ_{FRD} likely ranges from $(0.827)(\hat{\tau}_{FRD})$ to $\hat{\tau}_{FRD}$, where the latter estimate assumes no false-positives.

The covariates include sets of dummy variables for calendar year, state, industry, and an indicator of union activity. State and industry are reported in the ODI using the Standard Industrialization Classification codes (SIC). Using the SIC codes, industry is categorized into three groups: manufacturing (SIC 20 to 39), health services (SIC 80), and other. To obtain information on union activity, the ODI data are merged to "notices of bargaining" filed with the Federal Mediation and Conciliation Service (FMCS). A notice must be filed to modify a union contract and thus indicate union activity within an establishment. The FMCS data include all notices filed from 2004 to 2016. Using the FMCS data, the union indicator variable equals one if there is any union activity from 2004 to 2016 and zero otherwise. It should be noted that not all union establishments are expected to have filed with the FMCS during the data period, so union status is measured with error, particularly with false-negative errors.[37]

[36] The administrative records contain a list of establishments that are known to have been inspected under the SST plan, but the list is not comprehensive and does not report inspections outside of the SST plan.

[37] In the Appendix, we compare union status information in the IMIS to the match rate to the FMCS. We find that a match to the FMCS is highly correlated with union status.

5.5. Sample Summary

We initially focus on the DART cutoff for the primary inspection list. This cutoff is located near the top of the DART rate distribution. In column one of Table 1, the mean DART rate was 7.33, and the mean cutoff was 13.67. In columns two and three, the sample is split between establishments above and below the DART rate cutoff for the primary inspection list. According to the number of observations, only 14.08 percent of establishments exceeded the cutoff. The distribution of the DART rate relative to the cutoff is illustrated in Figure 1. As shown, the distribution is skewed to the right.

According to Table 1, the likelihood of a programmed inspection was greater above the cutoff than below: 30.3 percent and 5.1 percent, respectively. However, this difference pertains to all establishments above and below the cutoff, not at the cutoff. To illustrate the change at the cutoff, Figure 2 plots the likelihood of a programmed inspection by the DART rate relative to the cutoff. The markers denote the mean outcome within intervals of 0.5, and the lines are derived from local linear regression, estimated separately above and below the cutoff. As shown, the increase in inspections occurs at cutoff, as required for identification using the FRD design. Additionally, the increase in programmed inspections led to greater rates of citations and penalties (Figure 2). In contrast, the likelihood of an unprogrammed inspection, which is unrelated to the SST plan, did not change at the cutoff (Figure 2).

The FRD model assumes that, despite the discontinuity in inspections, the conditional mean functions $E[Y(0)|X_i = x]$ and $E[Y(1)|X_i = x]$ are continuous. This assumption is supported by two observations. First, the density of the DART rate is smooth near the cutoff, as shown in Figure 1.[38] This suggests that establishments do not bunch just below the cutoff to avoid inspection. This seems reasonable, since establishments report their DART rates before the SST cutoffs are determined.[39] It also suggests that inspections did not affect firm survival. For example, if inspections negatively affected firm survival, then then the density would be greater just below the cutoff.

Second, establishments appear similar just above and below the SST cutoff with respect to observable characteristics. According to column one of Table 1, approximately 61.0 percent of establishments are in manufacturing, 17.5 percent are in health services, and 12.5 percent exhibit union activity, according to FMCS data. Figure 3 plots these characteristics relative to DART rate, which show no measurable change at the cutoff.[40] The figure also plots the likelihood of an inspection during the year before the SST cycle. As shown, there is no discontinuity in the likelihood at the cutoff.[41]

Table 1 also shows that the case rates four years later are substantially lower than the baseline case rates, denoted by the subscripts $t + 4$ and t, respectively. Among all establishments, the TCR decreases from 12.8 to 9.5, and the DART decreases from 7.3 to 5.7.

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[38] A test by McCrary (2008) rejects that there is a discontinuity in the distribution at the cutoff. The smoothness at the cutoff also suggests that the increase in inspections did not affect firm survival. We also find no evidence of selection into the ODI sample or the analysis sample, particularly at the cutoff. First, the distribution of the DART rate relative to the cutoff among the entire ODI sample is smooth near the cutoff (Appendix Figure 1). Second, the likelihood of matching to an ODI observation four years later, required for the analysis sample, does not change discontinuously at the cutoff (Appendix Figure 2).

[39] In some years, the cutoffs remained unchanged (Appendix Table), allowing establishments to form expectations of the cutoffs over time. As a robustness check, we limit the analysis to establishments first observed when a new SST cutoff was implemented.

[40] Using local linear regression, the changes in these characteristics at the cutoff are small and statistically insignificant.

[41] Using local linear regression, the discontinuity in the likelihood of an inspection during the calendar year before the first observation in the ODI is .007 percent and statistically insignificant.

These decreases are greater among establishments above the cutoff: the TCR decreases from 27.0 to 14.4, and the DART decreases from 19.1 to 9.5. This is consistent with mean reversion in case rates (Ruser 1995), particularly at the top of the case rate distribution. This is also consistent with a general decrease in case rates over time.[42] These factors should not invalidate the identification strategy, however, if their impacts are similar above and below the cutoff.

4. Results

5.1. Inspections

The first step is to estimate the discontinuity in inspections at the cutoff. Panel A of Table 2 presents the estimated discontinuity and the optimal bandwidth using local linear regression without covariates. As shown, the cutoff is associated with a 22.7 percentage point increase in programmed inspections, a 17.6 percentage point increase in citations, and a 15.7 percentage point increase in penalties. These estimates are statistically significant at the one percent level and robust to the inclusion of covariates, as shown in panel B.

The final column of Table 2 presents the results for unprogrammed inspections, which were not directly affected by the SST plan. As expected, there is no discontinuous change in unprogrammed inspections at the cutoff.

The nature and severity of the citations and penalties are examined using the FRD estimand in equation (2), where the treatment variable is a programmed inspection and the outcome variable is the number of citations or the penalty amount.[43] Among compliers at the cutoff, a programmed inspection increased the average penalty by $6,156 in 2009 dollars, with a

[42] Among the full sample, the mean TCR decreased from 13.17 in 1996 to 6.23 in 2011.
[43] The model includes the full set of control variables: year fixed effects, state fixed effects, industry fixed effects, and an indicator for union activity.

standard error of $1,011 (not shown). Table 3 presents the results for the number of all citations and of the nine most common citations among the analysis sample. The most common citations are associated with manufacturing, with the exception of "bloodborne pathogens". As shown in the table, a programmed inspection increased the number of all citations by 5.06 and the top nine citations combined by 1.34.

5.2. Mean Effects

The increase in programmed inspections at the cutoff is used to identify the effect of an inspection on worker safety. To examine this effect graphically, Figure 4 plots case rates in the first calendar year after the SST cycle. The first panel plots the TCR, and the second panel plots the DART. In both panels, the mean case rate appears to decrease discontinuously at the cutoff, suggesting that inspections improved worker safety.

The FRD estimand in equation (2) relates the change in case rates to the change in inspections, both measured at the cutoff. With the assumptions outlined above, the FRD estimand represents the causal effect of an inspection among compliers.

The left side of Table 4 presents the baseline estimates separately for the TCR and the DART. As shown, an inspection decreases both the TCR and the DART. However, the standard errors do not rule out a large range of effects, and only the effect on the DART is statistically significant. Without covariates, the estimated effect on the TCR is -0.569, and the estimated effect on the DART rate is -1.607.[44] Relative to the post-inspection DART rate near the cutoff of

[44] Mentioned above, false-negative matches of the ODI to the IMIS may lead to overestimating the effect of workplace inspections on worker safety. Using a bias factor of 1/0.827, the estimated effect on the TCR ranges from -0.471 to -0.569, and the estimated effect on the DART rate ranges from -1.329 to -1.607.

eight (Figure 4), the effect on the DART amounts to a decline of approximately 20 percent. The estimates are similar with the inclusion of covariates: -0.769 and -1.792, respectively.

The right side of Table 4 presents estimates using data from 1998 to 2007. This allows consideration of a third outcome, the DAFWII, which is only available for calendar years 2002 and beyond. As shown, all three estimated effects are negative, but only the effect on the DART is statistically significant. With covariates, the estimated effect on the DART is -2.068. The estimated effects on the TCR and the DAFWII are smaller in magnitude and statistically insignificant.

In both panels, the estimates for the DART rate are larger than the estimates for the TCR or the DAFWII. A possible mechanism is that inspections reduced the severity of cases involving job restrictions or transfers to require only medical attention beyond first aid. Cases involving job restrictions and transfers are included in the TCR and the DART, but not the DAFWII, and cases involving medical attention beyond first aid are included in the TCR, but not the DART or DAFWII. Thus, the proposed mechanism would decrease the DART rate more than the TCR and the DAFWII. However, the standard errors for all the estimated effects are large and thus do not rule out a wide range of effects.

5.3. Robustness to Bandwidth and Order of Polynomial

In Table 5, we examine the robustness of the baseline results with respect to the order of the polynomial and the bandwidth. In Table 4, the order of the polynomial is one, and the bandwidth is chosen optimally using the procedure developed by Calonico, Cattaneo, and Titiunik (2014). Under these specifications, and controlling for observable characteristics, the optimal bandwidth is 3.17, and the estimated effect of an inspection on the DART rate is -1.792.

In Table 5, the order of the polynomial varies across rows, from one to three, and the bandwidth varies across columns, from 50 percent to 150 percent of the optimal bandwidth 3.17. As shown, the estimated effect is negative in all specifications, ranging from -1.101 to -2.650. Moreover, the estimates are more statistically significant with either a narrow bandwidth and a lower-order polynomial or a large bandwidth and a higher-order polynomial. This makes sense intuitively, as a larger bandwidth requires a more flexible function form with respect to the running variable.

5.4. Alternative Samples

In Table 6, we examine the effect of an inspection on worker safety using alternative samples. The baseline estimate of -1.792 is reported in column one. In columns two and three, we consider longer lags between the first and second observations. In column two, the observations are spaced five years apart, and, in column three, the observations are spaced six years apart. The longer lag decreases the sample size, which may reflect that some establishments no longer exist. In both columns, the estimates are smaller, positive, and statistically insignificant. This suggests that the effect of an inspection on worker safety may be ephemeral. However, the larger standard errors, due in part to fewer observations, do not rule out a wide range of effects.

In columns four through six, we focus on establishments that are less able to anticipate the SST plan and the DART cutoff. In column four, the sample is restricted to establishments that are observed exactly twice, spaced four years apart. In column five, the sample is restricted to the earliest paired observation. In column six, the sample is restricted to the earliest paired observation in the first year a new cutoff was implemented. These restrictions decrease the sample size considerably, with only 13,101 observations in column six. Nonetheless, the estimates remain negative, though statistically insignificant, ranging from -1.109 to -1.973.

5.5. Distributional Effects

The effect of an inspection may vary across the post-inspection rate distribution, conditional on being near the 85th percentile pre-inspection. To explore this possibility, the distributional effects of an inspection are examined using equations (3) and (4). Equation (3) presents the CDF of compliers when treated, and equation (4) represents the CDF of counterfactual compliers when not treated. The equations are estimated separately for integers of $Y = y$, from zero to sixteen, using local linear regression.

Figures 5 and 6 illustrate the estimated distributional effects for the DART rate. The first panel in Figure 5 plots the estimates of $F_{Y(1)|\Omega}(y)$ and $F_{Y(0)|\Omega}(y)$, and Figure 6 plots their difference and its 95 percent confidence interval. As shown, the effect of inspections is concentrated at bottom of the DART distribution. Starting at $Y = 0$, the difference in the conditional CDFs is approximately 11 percentage points, which is statistically significant at the one percent level. The difference remains positive and statistically significant up to $Y = 8$, though the 95 percent confidence intervals widen substantially. The difference then converges towards zero near the 92nd percentile. At that point, the difference is approximately one percent and statistically insignificant. Thus, the effects of an inspection on the DART rate occur predominately below the 90th percentile of the post-inspection rate distribution, conditional on being near the 85th percentile pre-inspection.

5.6. Effects by Industry

The effect of an inspection may also differ by industry. Differences may arise due to different occupational hazards, effective regulatory standards, and scopes for improvement. To explore this possibility, the models are estimated separately for manufacturing, health services, and "other" industries, with the DART rate as the outcome. Table 7 presents mean effects, and Figure 5 illustrate distributional effects. For brevity, the estimates of $F_{Y(1)|\Omega}(y)$ and $F_{Y(0)|\Omega}(y)$ are plotted, but not their differences.

According to the results, the effect of an inspection on worker safety is most evident for manufacturing, particularly below the 90th percentile. In regards to the mean effect, the estimate for manufacturing is -1.050 per 100 full-time equivalent workers, compared to 0.626 for health services and -0.124 for "other" industries. However, none of the mean estimates is statistically significant. In regards to distributional effects in manufacturing, there are sizeable differences between the conditional CDFs up to the 90th percentile, most of which are statistically significant. In contrast, there are no statistically significant differences in the conditional CDFs in health services or "other" industries.

5. Additional Considerations

5.1. Secondary Inspection List and Letter

Thus far, the empirical analysis has focused on the DART cutoff for the primary inspection list. However, stated above, a lower set of cutoffs defined a secondary inspection list, and an even lower cutoff determined which establishments received a letter stating that their case rate was high relative to the national average. An important consideration is whether these cutoffs affected the likelihood of an inspection or worker safety.

Regarding the secondary inspection list, the DART cutoff is associated with a small increase in programmed inspections, but there is no measurable change in worker safety. These findings are illustrated in Figure 7. Using local linear regression with covariates, the discontinuity in programmed inspections is 3.76 percentage points, which is statistically significant at the five percent level, but the change in the DART rate is 0.074, with a standard error of 0.127.[45] Similarly, the cutoff for the letter is associated with a small increase in programmed inspections, but there is no measurable change in worker safety. These findings are illustrated in Figure 8. Using local linear regression with covariates, the discontinuity in programmed inspections is 1.47 percentage points, which is statistically significant at the five percent level, but the change in the DART rate is 0.201, with a standard error of 0.121. Thus, alternative cutoffs are not associated with a substantial increase in programmed inspections or a change in worker safety.

5.2. ODI Data Recorded in 1996 and Collected in 1997

ODI data were recorded in 1996 and collected in 1997, but these data were not used to implement the SST plan. Thus, as a placebo test, the empirical analysis is repeated for the ODI data recorded in 1996 as if the SST plan had been implemented. The same sample restrictions are imposed on these data, including limiting the analysis to states under federal jurisdiction. Establishments observed in 1996 are assigned the DART cutoff for the primary inspection list as if they were observed in 1997.

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[45] A secondary inspection list was not specified in some years, so the sample size is reduced to 137,848.

As expected, the data reveal no discontinuity in either programmed inspections or the DART rate. These findings are illustrated in Figure 9. Using local linear regression, the discontinuity in programmed inspections is 3.56 percentage points, with a standard error of 4.66, and the discontinuity in the DART rate is -0.496, with a standard error of 0.711.

5.3. Non-Participating States

ODI data were collected in 45 US states and the District of Columbia, but the SST plan was only implemented in 35 states. Thus, as a placebo test, the empirical analysis is repeated for states where ODI data were collected, but the SST plan had not been implemented. Establishments in non-participating states are assigned the DART cutoff for the primary inspection list as if they resided in states that implemented the SST plan.

Again, the data reveal no discontinuity in either programmed inspections or the DART rate. These findings are illustrated in Figure 10. Using local linear regression with covariates, the discontinuity in programmed inspections is -0.062 percentage points, with a standard error 1.12, and the discontinuity in the DART rate is -0.283, with a standard error of 0.284. Thus, as expected, the DART cutoff is not associated with a change in programmed inspections or worker safety in states that are not covered by the SST plan.

6. Conclusion

This study examines the effect of an OSHA inspection on worker safety. To identify the effect, the study exploits quasi-experimental variation in inspections due to OSHA's SST plan. The effect is identified specifically among establishments near the 85th percentile of the DART rate distribution pre-inspection that were inspected as a result of the SST plan. Using the fuzzy regression discontinuity design and local linear regression, the causal effect of an inspection on the DART rate is approximately -1.792 per 100 full-time equivalent workers. Relative to the mean, this effect is a reduction of approximately 20 percent. The effect is most evident for manufacturing establishments below the 90th percentile of the DART rate distribution post-inspection.

The estimated effect of an OSHA inspection on worker safety found in this study is large compared to related studies. As noted, most studies find little to no effect of inspections on worker safety, and studies that do find effects may suffer from statistical biases or lack generalizability. However, it is difficult to reconcile this study to most related studies, since they differ in regards to identification strategy, data, population of interest, and worker safety outcomes.

Regarding efficiency, an important question is whether the gains from the additional inspections exceed the marginal costs. According to Viscusi and Aldy (2003), the value of statistical injury ranges from $20 thousand to $70 thousand. If equated to the DART, the mean effect of an inspection on the DART rate of -1.792 ranges in value from $35.8 thousand to $125.4 thousand annually per 100 full-time equivalent workers. This range represents the average private gain of an inspection and excludes fiscal externalities through, for example, social insurance programs. The marginal cost includes the cost of an inspection, which equaled $6.5 thousand on average in 2016, as well as compliance costs to employers.[46] Although compliance costs are difficult to estimate, they can be bounded to determine efficiency.[47] For

[46] Financial penalties are direct monetary transfers from establishments to OSHA and thus do not affect social welfare.
[47] The average cost of an inspection is derived by dividing the total OSHA budget on federal enforcement of $208 million by the number of federal OSHA inspections of 31,948. For a thorough discussion on compliance costs, see Kniesner and Leeth (2014).

example, in an establishment of 40 employees, the minimum establishment size in the ODI, an inspection would inefficient if compliance costs exceeded roughly $7.8 thousand to $43.7 thousand plus the external gains from reducing workplace injuries.[48] To improve efficiency, OSHA should target establishments for inspection in which the effect on worker safety is greatest. In this study, the effect is most evident in manufacturing and less evident in health services, the largest two-digit industries represented in the ODI data.

[48] This calculation assumes that the average cost of an inspection for an establishment with 40 employees is equal to $6.5 thousand, the average cost of an inspection among all establishments in 2016. However, it is likely that that the costs of an inspection and compliance increase with establishment size. In 2016, the median size of establishments inspected by OSHA was 11.

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Appendix

This study uses establishment-level data from OSHA's Data Initiative (ODI) matched to records on notices of union bargaining from the Federal Mediation and Conciliation Service (FMCS) and inspection records from the Integrated Management Information System (IMIS). This section provides the procedures used to link these datasets.

The analysis sample is derived from the ODI, which provides establishment-level data on accidents and injuries. The sample is limited to establishments observed at least twice, with the two observations spaced exactly four years. To link multiple observations of an establishment across years, the establishment name and address were standardized. All special characters, such as @, #, and /, were removed. For the establishment name, common words such as Company, Corporation, and Co, were deleted. Some establishments operated under a different name as their parent company, often indicated by DBA, an acronym for, doing business as. In these cases, the establishment name is separated into two, with the second name as a new variable. For the establishment address, floor numbers, suite numbers, and room numbers were removed. Common words such as Street, Road, and Avenue are standardized to abbreviations St, Rd, and Ave. For city names, we construct a list of all the city-state combinations that appear in ODI and matched them to a list of city names from Census. Any city-state combinations with no match to the list were checked manually for errors in either the state or the spelling of the city. Duplicates of the same establishment (based on the identifier we generated) in the same year are deleted (less than one percent of the sample).

The ODI data are then linked to the inspection data during the SST cycle from the Integrated Management Information System (IMIS). IMIS includes the universe of the inspections conducted by OSHA from 1970 and reports the name and address of the inspected establishments, including street address, zip code, city, and state, which are used to link ODI data. The establishment name and address are standardized using the same method used to standardize the ODI data. The ODI data are then matched to the IMIS data using five criteria. If an establishment is matched successfully based on one criterion, the establishment and its inspection record are removed from subsequent matching. First, establishments matched based on the establishment name and street address within the same city and state. Second, the first criteria is repeated using the second name, if applicable. Third, establishments are matched based on establishment name and 5-digit zip code within the same city and state. Fourth, establishments are matched based on the first six letters of the establishment name and street address (excluding spaces). Fifth, establishments are matched based on street address within the same city and state, after manually verifying a match of the establishment name, and on establishment name, after manually verifying a match on the street address. Among establishments with a match, 57 percent match using the first criteria, two percent match using the second criteria, 16 percent match using the third criteria, 18 percent match using the fourth criteria, and seven percent match using the fifth criteria.

The ODI data are also linked to the universe of notices of bargaining filed with Federal Mediation and Conciliation Service (FMCS). The universe of notices are available from 2004 to 2016. Because unions must file with the FMCS to modify an existing contract, a notice indicates whether any collective bargaining activity occurs within an establishment (DiNardo and Lee, 2004). Again, the establishment name and address are standardized, and the ODI data are matched to the FMCS data using several criteria. An establishment is assumed unionized if there is any match to a record in FMCS. This assumption can be checked among establishments matched to both the FMCS and the IMIS, since the inspection data also report whether the establishment is unionized. Among these establishments, 89.3 percent with a match to the FMCS are unionized according to the IMIS, and only 10.8 percent without a matched to the FMCS are unionized. Thus, a match to the FMCS is highly correlated with union status.

OSHA's Form 300 (Rev. 01/2004)

Log of Work-Related Injuries and Illnesses

Attention: This form contains information relating to employee health and must be used in a manner that protects the confidentiality of employees to the extent possible while the information is being used for occupational safety and health purposes.

Year 20__ __

U.S. Department of Labor
Occupational Safety and Health Administration

Form approved OMB no. 1218-0176

You must record information about every work-related death and about every work-related injury or illness that involves loss of consciousness, restricted work activity or job transfer, days away from work, or medical treatment beyond first aid. You must also record significant work-related injuries and illnesses that are diagnosed by a physician or licensed health care professional. You must also record work-related injuries and illnesses that meet any of the specific recording criteria listed in 29 CFR Part 1904.8 through 1904.12. Feel free to use two lines for a single case if you need to. You must complete an Injury and Illness Incident Report (OSHA Form 301) or equivalent form for each injury or illness recorded on this form. If you're not sure whether a case is recordable, call your local OSHA office for help.

Establishment name ________________

City __________ State ________

Identify the person			**Describe the case**			**Classify the case** CHECK ONLY ONE box for each case based on the most serious outcome for that case:				**Enter the number of days the injured or ill worker was:**		**Check the "Injury" column or choose one type of illness:**					
								Remained at Work				(M)					
(A) Case no.	(B) Employee's name	(C) Job title (*e.g., Welder*)	(D) Date of injury or onset of illness	(E) Where the event occurred (*e.g., Loading dock north end*)	(F) Describe injury or illness, parts of body affected, and object/substance that directly injured or made person ill (*e.g., Second degree burns on right forearm from acetylene torch*)	(G) Death	(H) Days away from work	(I) Job transfer or restriction	(J) Other record-able cases	(K) Away from work	(L) On job transfer or restriction	(1) Injury	(2) Skin disorder	(3) Respiratory condition	(4) Poisoning	(5) Hearing loss	(6) All other illnesses
___	___	___	__/__ month/day	___	___	☐	☐	☐	☐	__ days	__ days	☐	☐	☐	☐	☐	☐
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					Page totals ➤	___	___	___	___	___	___	___ (1) Injury	___ (2) Skin disorder	___ (3) Respiratory condition	___ (4) Poisoning	___ (5) Hearing loss	___ (6) All other illnesses

Be sure to transfer these totals to the Summary page (Form 300A) before you post it.

Public reporting burden for this collection of information is estimated to average 14 minutes per response, including time to review the instructions, search and gather the data needed, and complete and review the collection of information. Persons are not required to respond to the collection of information unless it displays a currently valid OMB control number. If you have any comments about these estimates or any other aspects of this data collection, contact: US Department of Labor, OSHA Office of Statistical Analysis, Room N-3644, 200 Constitution Avenue, NW, Washington, DC 20210. Do not send the completed forms to this office.

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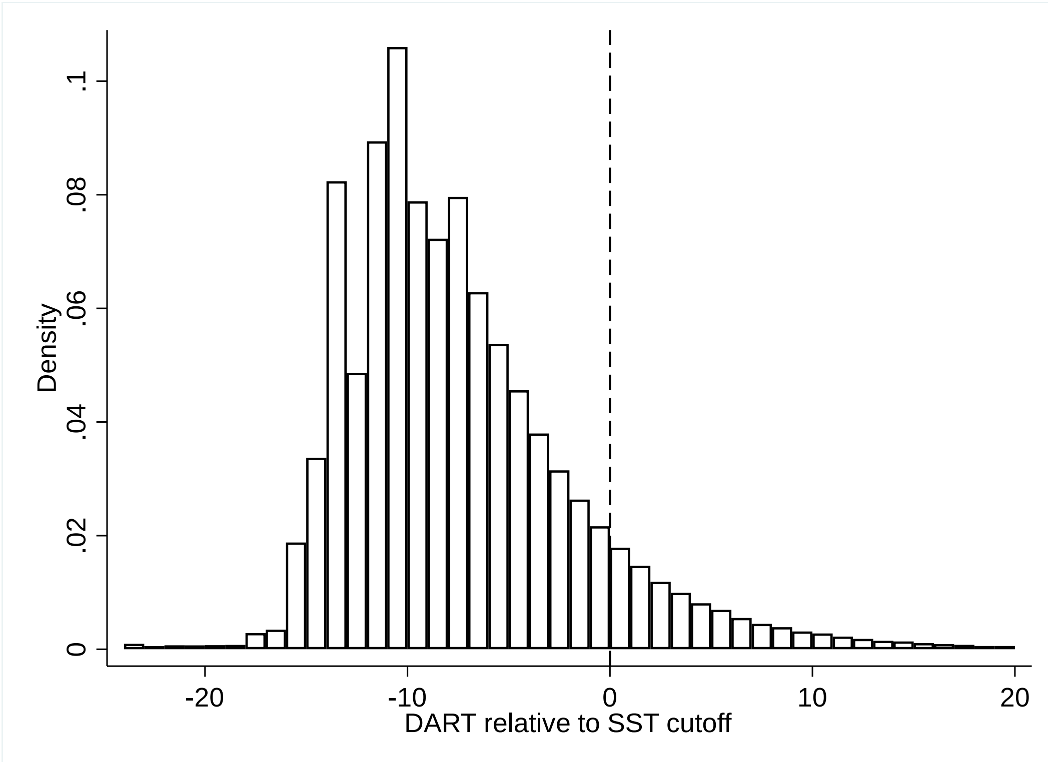
Appendix Table: SST Timing and Cutoffs

ODI Data		SST Cycle		ODI	Primary Inspection List					
					DART			DAFWII		
Recorded	Collected	Begin	End	Outcome	SIC 20-39	SIC 80	Other	SIC 20-39	SIC 80	Other
1996	1997	-	-	2000	-	-	-	-	-	-
1997	1998	19-Apr-99	4-Feb-00	2001	16	24	16	-	-	-
1998	1999	4-Feb-00	13-Jul-01	2002	14	14	14	-	-	-
1999	2000	13-Jul-01	13-Jul-02	2003	14	14	14	-	-	-
2000	2001	7-Jul-02	10-Jun-03	2004	14	-	14	-	-	-
2001	2002	10-Jun-03	19-Apr-04	2005	14	17	14	9	-	9
2002	2003	19-Apr-04	5-Aug-05	2006	15	17.75	15	10	-	10
2003	2004	5-Aug-05	12-Jun-06	2007	12	14.65	12	9	-	9
2004	2005	12-Jun-06	14-May-07	2008	12	15.15	12	9	-	9
2005	2006	14-May-07	19-May-08	2009	11	14.17	11	9	-	9
2006	2007	19-May-08	20-Jul-09	2010	11	13.7	11	9	-	9
2007	2008	20-Jul-09	22-Oct-10	2011	8	17	15	6	14	13
2008	2009	22-Oct-10	9-Sep-11	-	7	16	15	-	-	-
2009	2010	9-Sep-11	4-Jan-13	-	7	16	15	5	13	14
2010	2011	4-Jan-13	2-Feb-14	-	7	-	15	5	-	14
2011	2012	2-Feb-14	2-Feb-15	-	7	-	15	5	-	14

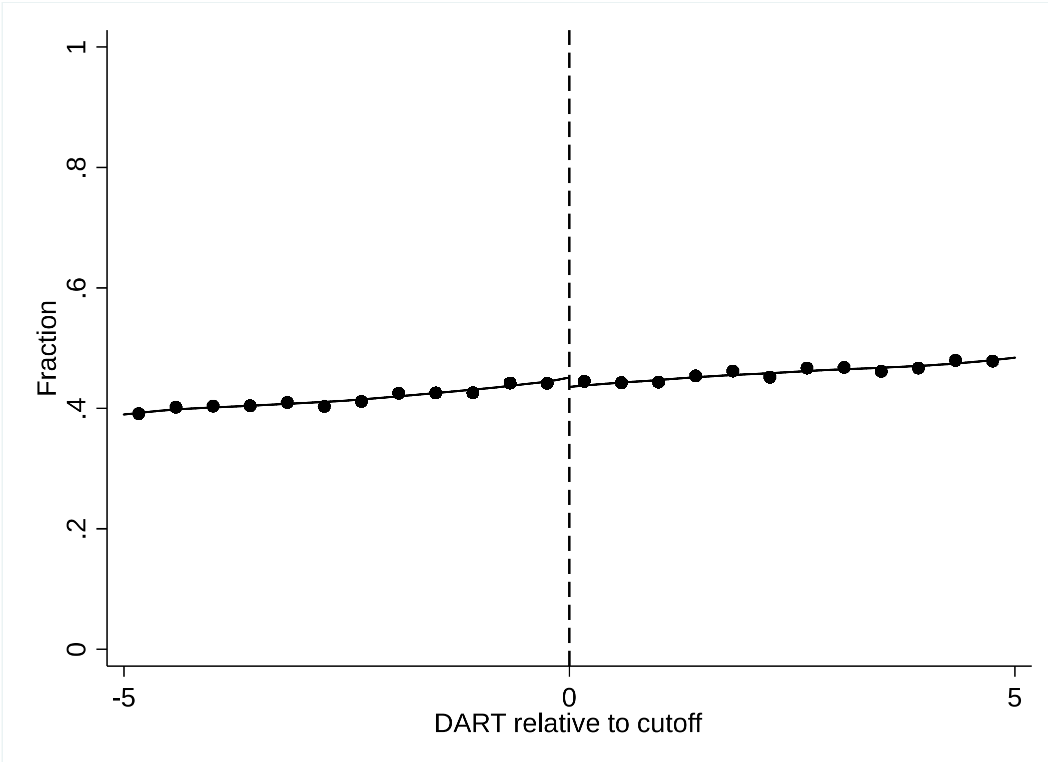
Appendix Table (continued): SST Timing and Cutoffs

ODI Data		Secondary Inspection List						
		DART			DAFWII			
Recorded	Collected	SIC 20-39	SIC 80	Other	SIC 20-39	SIC 80	Other	Letter
1996	1997	-	-	-	-	-	-	-
1997	1998	10	-	10	-	-	-	8
1998	1998	8	8	8	-	-	-	8
1999	1998	8	8	8	-	-	-	8
2000	1998	8	8	8	-	-	-	8
2001	1998	8	8	8	4	4	4	6
2002	1998	8	-	8	4	-	4	7
2003	1998	7	-	7	5	-	5	6.5
2004	1998	7	-	7	5	-	5	6
2005	1998	7	-	7	4	-	4	5.3
2006	1998	7	-	7	5	-	5	5.4
2007	1998	6	15	6	4	11	4	5
2008	1998	5	13	7	4	11	5	4.5
2009	1998	5	13	5	4	11	4	2.5
2010	1998	5	-	7	4	-	5	2
2011	1998	5	-	7	4	-	5	-



Appendix Figure 1: Distribution of DART case rate relative to SST Cutoff

The sample is derived from OSHA's Data Initiative. The sample consists of all establishments from 1997 to 2007.



Appendix Figure 2: Likelihood of ODI Observation Four Years Later

The sample is derived from OSHA's Data Initiative. The sample consists of all establishments from 1997 to 2007.

Table 1. Summary Statistics

	All	DART<Cutoff	Dart>=Cutoff
TCR t	12.81	10.49	26.96
	(0.03)	(0.02)	(0.08)
DART t	7.33	5.41	19.06
	(0.02)	(0.01)	(0.05)
Cutoff	13.67	13.71	13.41
	(0.01)	(0.01)	(0.02)
Inspection, programmed	0.086	0.051	0.303
	(0.001)	(0.001)	(0.003)
Citation, programmed	0.064	0.036	0.230
	(0.001)	(0.001)	(0.003)
Penalty, programmed	0.057	0.033	0.203
	(0.001)	(0.000)	(0.003)
Inspection, unprogrammed	0.046	0.044	0.062
	(0.001)	(0.001)	(0.002)
Manufacturing	0.610	0.622	0.536
	(0.001)	(0.001)	(0.003)
Health services	0.175	0.177	0.166
	(0.001)	(0.001)	(0.003)
Union activity	0.125	0.122	0.143
	(0.001)	(0.001)	(0.002)
TCR t+4	9.51	8.71	14.38
	(0.02)	(0.02)	(0.06)
DART t+4	5.69	5.08	9.47
	(0.01)	(0.01)	(0.05)
Establishments	61,702	55,247	6,455
Observations	154,808	133,013	21,795

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The TCR is the rate of cases involving death, days away from work, job transfers and restrictions, and medical attention beyond first aid; the DART includes cases involving days away from work and job transfers and restrictions; the DAFWII includes cases involving days away from work. All rates are measured per 100 full-time employees. The subscript t denotes the first of the two observations; the subscript t+4 denotes the second. The cutoff is the DART rate cutoff for the primary inspection list. The inspection outcomes come from OSHA’s IMIS.

Table 2. Discontinuity in Inspection

Inspection Type	Programmed	Citation	Penalty	Unprogrammed
A. Without covariates				
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.227***	0.176***	0.157***	0.005
	(0.013)	(0.011)	(0.011)	(0.006)
Bandwidth h	3.57	3.68	3.55	6.06
B. With covariates				
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.224***	0.174***	0.155***	0.006
	(0.012)	(0.011)	(0.011)	(0.006)
Bandwidth h	3.62	3.74	3.63	4.76
Observations	154,808	154,808	154,808	154,808

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with inspection as the outcome variable. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union activity. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{Dl} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 3. Effect of Inspection on Citations

Citation category	$\hat{\tau}_{FRD}$
All citations	5.063***
	(0.367)
The control of hazardous energy (lockout/tagout)	0.199***
	(0.026)
Wiring design and protection	0.213***
	(0.026)
General requirements for all machines	0.199***
	(0.028)
Electrical, general	0.184***
	(0.026)
Hazard communication	0.167***
	(0.025)
Respiratory protection	0.097***
	(0.016)
Mechanical power-transmission apparatus	0.102***
	(0.021)
Abrasive wheel machinery	0.069***
	(0.021)
Bloodborne pathogens	0.111***
	(0.018)

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with the number of citations as the outcome variable and a programmed inspection as the treatment variable. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union activity. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 4. Effect of Inspection on Worker Safety

ODI Year	1997-2007		1998-2007		
Outcome Variable	TCR	DART	TCR	DART	DAFWII
A. Without covariates					
$\hat{\tau}_{FRD}$	-0.569	-1.607**	-1.294	-1.877**	-0.511
	(1.143)	(0.787)	(1.121)	(0.844)	(0.639)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.227***	0.227***	0.217***	0.218***	0.217***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)
Bandwidth h	3.65	3.57	3.90	3.56	3.63
B. With covariates					
$\hat{\tau}_{FRD}$	-0.769	-1.792**	-1.717	-2.068**	-0.554
	(1.150)	(0.814)	(1.215)	(0.872)	(0.607)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.224***	0.224***	0.215***	0.215***	0.215***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Bandwidth h	3.45	3.17	3.25	3.13	3.57
Observations	154,808	154,808	139,220	139,220	139,220

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with the case rate as the outcome variable and a programmed inspection as the treatment variable. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union activity. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{Dl} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 5. Effect of Inspection on Worker Safety by Bandwidth and Order of Polynomial

	Order of Polynomial	Bandwidth 50%	75%	100%	125%	150%
$\hat{\tau}_{FRD}$	1	-2.446**	-1.880*	-1.792**	-1.508**	-1.101
		(1.235)	(0.968)	(0.814)	(0.762)	(0.708)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$		0.208***	0.224***	0.224***	0.227***	0.226***
		(0.018)	(0.015)	(0.013)	(0.012)	(0.011)
$\hat{\tau}_{FRD}$	2	-1.903	-2.489**	-2.144*	-2.026**	-2.129**
		(1.829)	(1.403)	(0.156)	(1.012)	(0.922)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$		0.187***	0.204***	0.217***	0.223***	0.226***
		(0.025)	(0.020)	(0.018)	(0.016)	(0.015)
$\hat{\tau}_{FRD}$	3	-2.360	-2.268	-2.650	-2.306*	-2.056*
		(2.275)	(1.957)	(1.620)	(1.371)	(1.207)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$		0.192***	0.183***	0.194***	0.207***	0.216***
		(0.031)	(0.026)	(0.022)	(0.020)	(0.018)
Bandwidth h		1.58	2.37	3.17	3.96	4.75
Observations		154,808	154,808	154,808	154,808	154,808

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with the case rate as the outcome variable and a programmed inspection as the treatment variable. The bandwidths are relative to the baseline model with covariates for DART in Table 4. At 100 percent, the bandwidth equals 3.17. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union activity. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{Dl} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 6. Effect of Inspection on Worker Safety, Alternative Samples

	DART					
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\tau}_{FRD}$	-1.792**	0.064	0.336	-1.292	-1.109	-1.973
	(0.814)	(0.938)	(0.933)	(1.954)	(1.153)	(2.426)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.224***	0.219***	0.214***	0.218***	0.275***	0.258***
	(0.013)	(0.014)	(0.014)	(0.034)	(0.022)	(0.046)
Bandwidth h	3.17	3.29	4.26	7.00	4.49	3.61
Observations	154,808	125,245	103,514	25,460	61,702	13,101

The sample is derived from ODI. The estimates come from a regression discontinuity model using local linear regression, with the case rate as the outcome variable and a programmed inspection as the treatment variable. Column 1 shows the main results as presented in Table 4, column 2; column 2 and column 3 show longer run results, measured two and three years after the SST plan; column 4 shows results among establishments observed exactly twice in t and t+4 in ODI; column 5 shows results using the earliest paired observation from t to t+4; and column 6 shows results using the earliest paired observation from t to t+4 when a new cutoff was implemented. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union activity. The bandwidths are relative to the baseline model with covariates for DART in Table 4. At 100 percent, the bandwidth equals 3.17. . The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{Dl} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

Table 7. Effect of Inspection on Worker Safety by Industry

	Industry		
	Manufacturing	Health Services	Other
$\hat{\tau}_{FRD}$	-1.050	0.626	-0.124
	(0.859)	(1.317)	(1.532)
$\hat{\alpha}_{DR} - \hat{\alpha}_{DL}$	0.208***	0.238***	0.245***
	(0.013)	(0.022)	(0.024)
Bandwidth h	6.37	6.11	3.07
Observations	94,410	27,136	33,262

The sample is derived from ODI. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The estimates come from a regression discontinuity model using local linear regression, with the case rate as the outcome variable and a programmed inspection as the treatment variable. The data on inspections come from OSHA's IMIS. The covariates include year fixed effects, state fixed effects, industry fixed effects (manufacturing, health services, and other), and an indicator of union activity. The parameter α_{DR} represents the mean outcome just above the SST cutoff; the parameter α_{DL} represents the mean outcome just below the SST cutoff. ***, **, and * indicate statistical significance at the one, five, and ten percent levels, respectively.

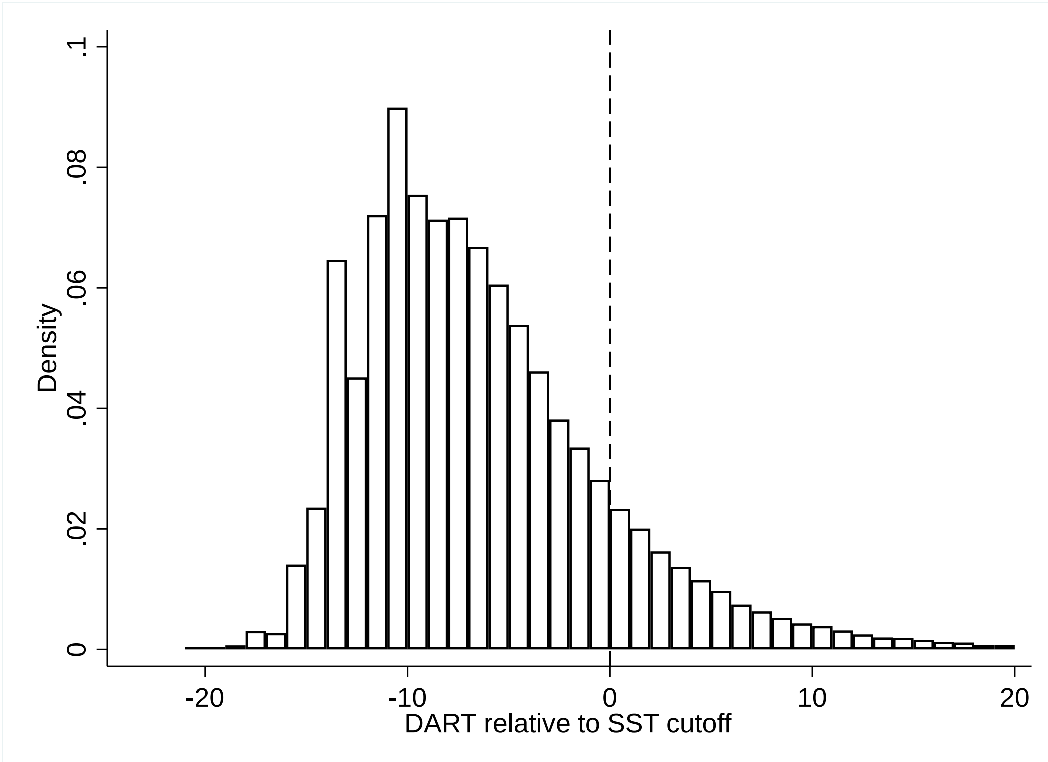


Figure 1. Distribution of DART case rate relative to SST Cutoff

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART is the rate of cases involving days away from work and job transfers or restrictions per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list.

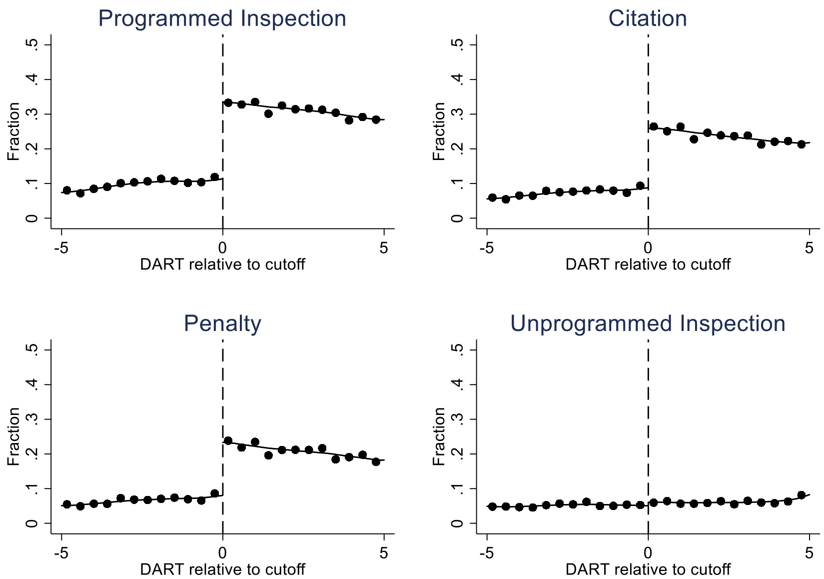


Figure 2. Inspections by DART relative to SST Cutoff

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART is the rate of cases involving days away from work and job transfers or restrictions, per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list. The inspection outcomes are derived from OSHA's Integrated Management Information System. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

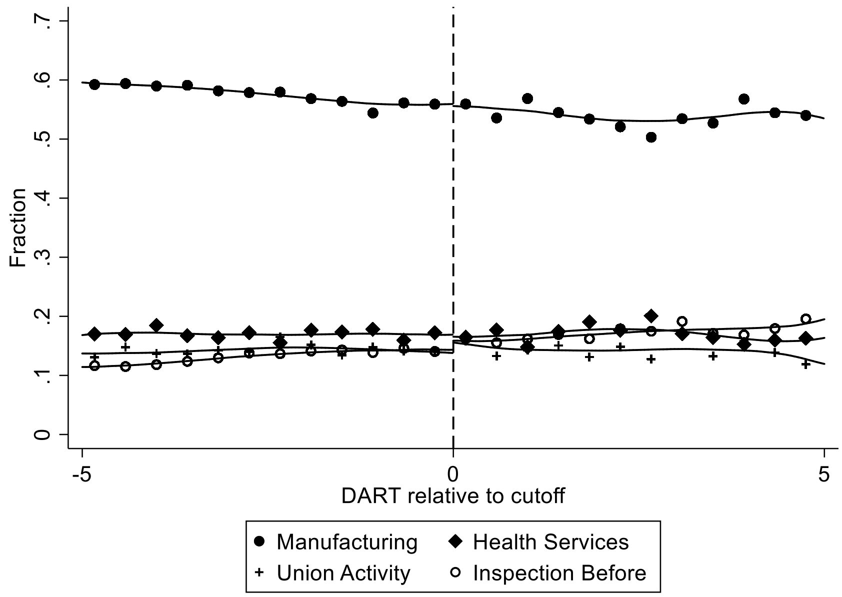


Figure 3. Establishment Characteristics by DART relative to SST Cutoff

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART is the rate of cases involving days away from work and job transfers or restrictions per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

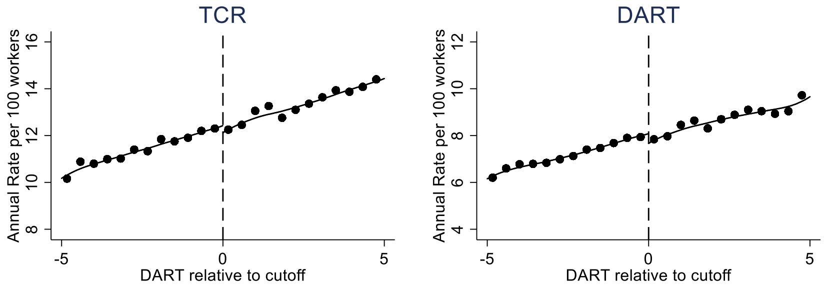


Figure 4. Case Rate Outcomes by DART relative to SST Cutoff

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The TCR includes cases involving death, days away from work, job transfers and restrictions, and medical treatment beyond first aid, and the DART includes cases involving days away from work and job transfers or restrictions, both measured per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

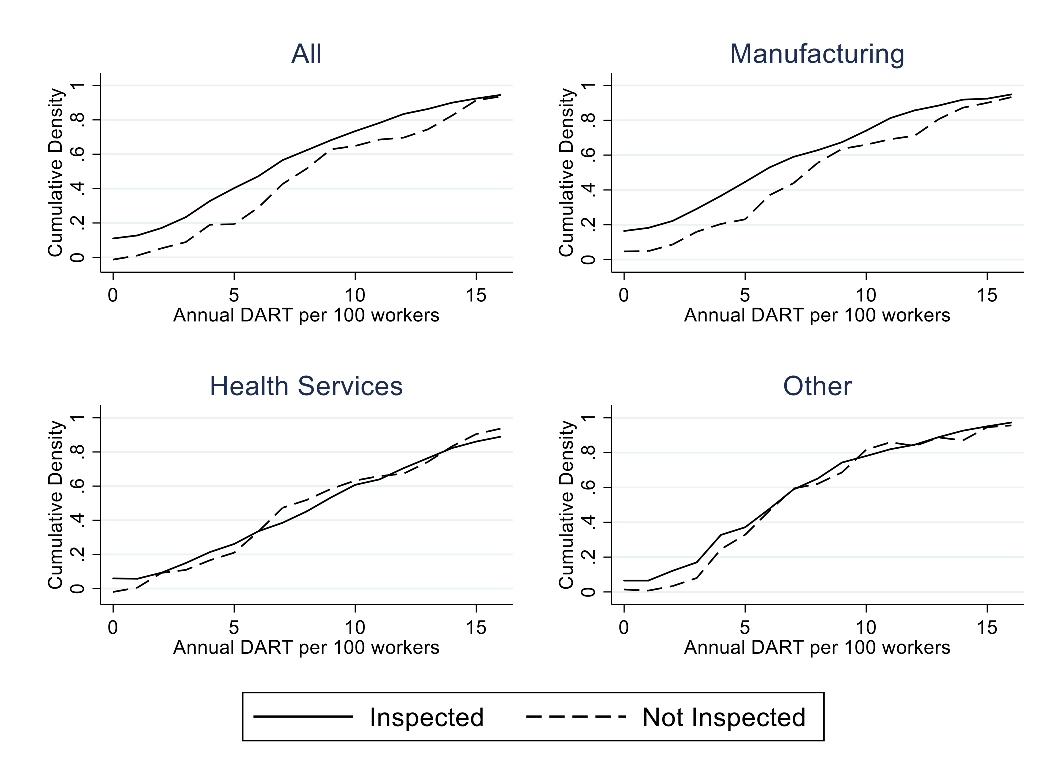


Figure 5. Distributional Effects of Inspection on DART Rate

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The panels plot the cumulative density functions of the DART rate among compliers just above the cutoff, that are inspected, and counterfactual compliers just below the cutoff, that are not inspected. The DART is the rate of cases involving days away from work and job transfers or restrictions per 100 full-time employees.

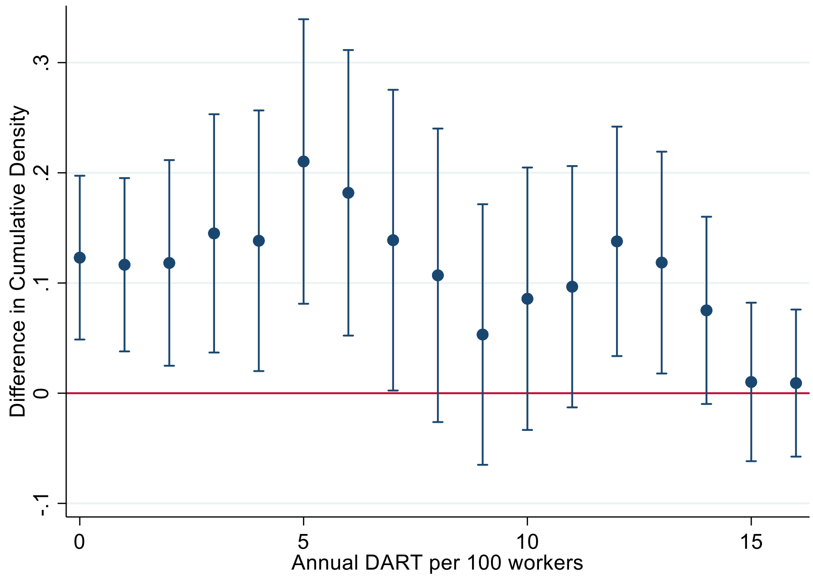


Figure 6. Distributional Effects of Inspection on DART Rate

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The figure plots the difference in the cumulative density functions plotted in the first panel of Figure 5. The DART is the rate of cases involving days away from work and job transfers or restrictions per 100 full-time employees.

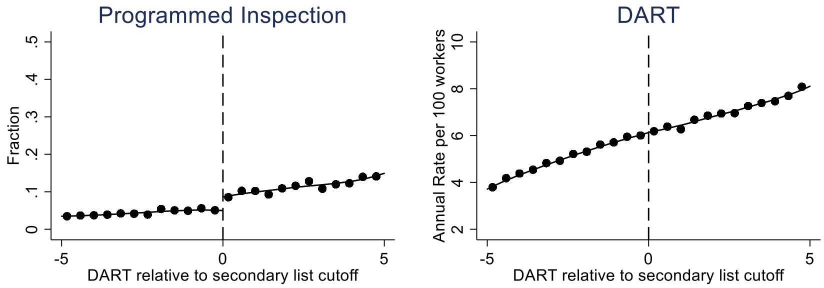


Figure 7. Secondary Inspection List

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART rate includes cases involving days away from work and job transfers or restrictions, per 100 full-time employees. Establishments are deleted if there is no secondary inspection list for the SST cycle. The x-axis is the DART rate from the first observation relative to the DART cutoff for the secondary inspection list. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

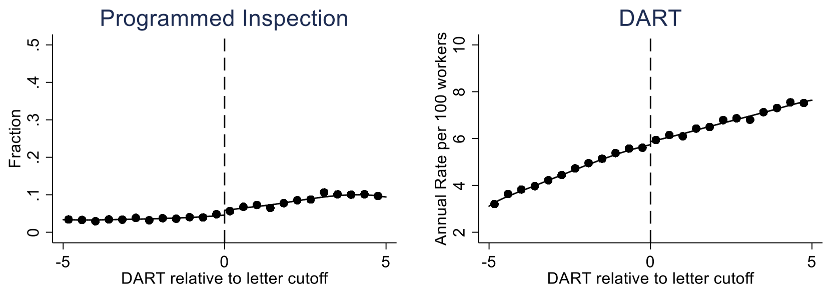


Figure 8. Letter List

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART rate includes cases involving days away from work and job transfers or restrictions per 100 full-time employees. The x-axis is the DART rate from the first observation relative to the cutoff for a letter. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

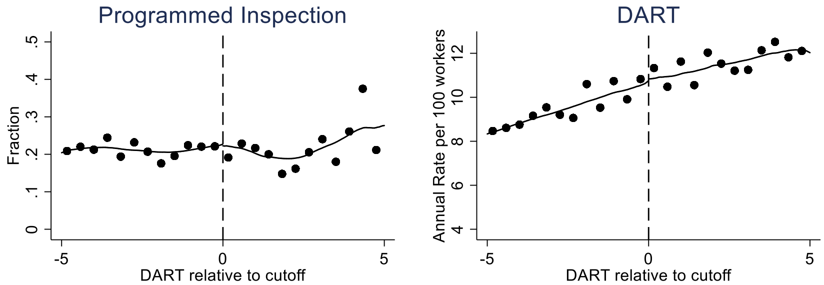


Figure 9. ODI Data Recorded in 1996 and Collected in 1997

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART rate includes cases involving days away from work and job transfers or restrictions per 100 full-time employees. The ODI Data Recorded in 1996 were not used by the SST plan. The x-axis is the DART rate from the first observation in 1996 relative to the DART cutoff for the primary inspection list in 1997. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

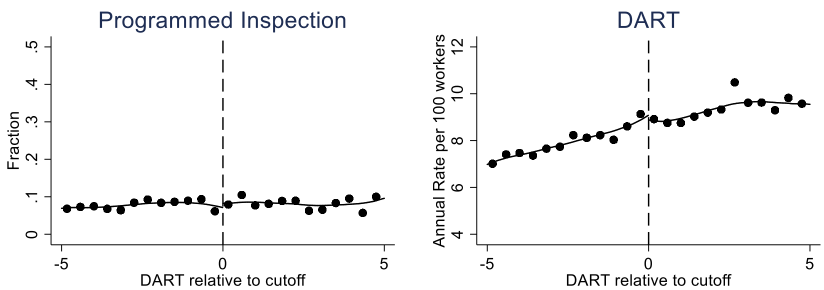


Figure 10. Non-Federal States

The sample is derived from OSHA's Data Initiative. The sample consists of establishments observed at least twice, with the two observations spaced four calendar years apart. The DART rate includes cases involving days away from work and job transfers or restrictions per 100 full-time employees. The SST plan was not implemented in non-federal states. The x-axis is the DART rate from the first observation relative to the DART cutoff for the primary inspection list in federal states. The markers denote the mean outcome within intervals of 0.5; the line is derived from local linear regression.

Chapter 3. Workplace Safety and Worker Productivity: Evidence from the MINER Act

1. Introduction

The coal mine industry has one of the highest workplace injury rates and also receives the most comprehensive regulations regrading working conditions. The extensive enforcement effort may lead to better compliance with safety standards and thus reduce workplace injuries. However, improving workplace safety may require workers to devote extra time or efforts on precautions, which may negatively affect worker productivity. Figure 1 plots the injury rate and the productivity in coal mines from 2000 to 2015. The injury rate decreased from 7.7 cases per 100 full-time equivalent employees in 2000 to 3.6 cases in 2015 while the miner productivity decreased from 6.91 short tons per employee hour to 6.14. The extensive enforcement effort in the mining sector may lead to fewer workplace injuries and also contribute to the decrease of worker productivity. This study provides empirical evidence on the impact of safety enforcement on workplace safety and worker productivity in coal mines.

The empirical strategy uses the introduction of the high-penalty “flagrant violation” in the Mine Improvement and New Emergency Response Act (MINER Act) of 2006. The MINER Act significantly increased the penalties for all workplace safety violations and allowed the issuance of flagrant violations. A flagrant violation may lead to a maximum civil penalty of $220,000, much higher than the penalties for other violations, ranging from $112 to $70,000 per violation. As the employers determine the level of compliance by comparing the costs of complying with the regulations with the expected penalties for non-compliance (Polinsky and Shavell, 2000), a flagrant violation may change the employers’ expected costs of non-compliance, thus lead to better compliance and fewer injuries. Another advantage of focusing on coal mines is the activities of the miners, the production technology, and the quality of output are relatively stable over the past several decades. Thus, any observed changes in workplace safety and worker productivity are unlikely to be confounded by the changes in production technology or product quality.

The data of this study come from several datasets in the Mine Data Retrieval System (MDRS), maintained by the Mine Safety and Health Administration (MSHA). The data set includes coal mines that are active at any point since 1983 and reports their quarterly production and employment; and the details on the workplace injuries and inspections, violations, and penalties. The analysis period is from the first quarter of 2000 to the fourth quarter of 2016.

With an event study model, the results show the issuance of a flagrant violation led to an around 0.22 million increase of the total penalties in one quarter, representing a more than two hundred percent increase relative to the quarter right before the issuance of the flagrant violation. After a flagrant violation, the workplace injury rates, defined as the number of workplace injuries per 100 full-time equivalent employees, showed a decreasing trend. The injury rate is estimated to decrease by 0.182 cases during the first two years after the flagrant violation, and continued to decrease by an average 1.388 cases three to four years after the flagrant violation, representing a significant 20 percent decrease. The coefficients on periods prior to the flagrant violation are close to zero and insignificant, suggesting no pre-existing trend of injury rates led up to the flagrant violation. The decrease in injury rates is mainly driven by the decrease of injuries with days away or restricted from work while the rate of injuries involving permanent disabilities or deaths, and mild injuries with no losses of workdays showing small and insignificant change.

While flagrant violations led to substantial improvement of workplace safety, worker productivity, defined as the number of short tons of coal produced per labor hour, decreased right after the issuance of a flagrant violation. The productivity decreases by 0.25 ton per hour of labor input (4 percent) during the first two years after the flagrant violation and continued to decrease by 0.322 in average three to four years after. Overall, the results show that after a flagrant violation with a large amount of penalties, both injury rate and worker productivity decreased, which supports the assumption that government enforcement of safety standards leads to improved workplace safety but lower worker productivity.

The event-study model estimates the changes within mines before and after the issuance of a flagrant violation and show a decrease in injury rate and worker productivity. In addition to a structural change that affects all mines, the flagrant violations may affect workplace safety and worker productivity through a compositional effect that more dangerous or less productive mines exit the market (Neumann and Nelson, 1982). The effect of a flagrant violation on the survival of coal mines is tested separately. The mines are three percentage points more likely to exit within the first two years after the issuance of a flagrant violation.

This study provides new evidence on the effectiveness of the enforcement of safety regulations. Inspections and the associated penalties are the primary tools the government used to enforce workplace safety standards. Most of the previous literature finds inspections on workplace safety have a small and insignificant effect on workplace injuries (Kniesner and Leeth, 2014). A close study is Scholz and Gray (1990), which estimates the effect of penalties from OSHA on workplace injury rate and finds only a small effect. In contrast, this study uses the citation on the flagrant violation as an exogenous shock on penalties, which increased the quarterly penalty by 200 percent, and shows the injury rate decreased significantly afterwards.

This study adds to the literature on the economic costs of safety regulations (Hahn and Hird, 1991; Crain and Crain, 2010). Regulations on safety and health are commonly cited as a major cause of the productivity decrease in coal mines (Darmstadter, 1997). Using the estimates from the event-study model, the decrease in workplace injury rate is equivalent to a cost saving of $1.10 per labor hour while the decrease in labor productivity valued $1.46 per hour. Thus, the productivity loss is 1.3 times the benefits of injury reduction. The enforcement efforts, aiming at improving workplace safety, have generated higher losses in worker productivity compared to the gain from reduced injuries.

This study also contributes to the literature on the link between workplace safety and worker productivity. Consistent with the previous literature (Gray, 1987; Boal, 2017; Gowrisankaran et al., 2017), the results of this study suggest a trade-off between workplace safety and worker productivity. The advantage of this study is to use penal data on coal mine injuries and productivity and MSHA enforcement as an exogenous shock on workplace safety, which is unlikely to be confounded by any unobserved factors such as management skills and worker quality.

The rest of the paper is organized as follows: section 2 reviews the theory of public enforcement and the enforcement in the mining sector; section 3 presents the empirical strategy; section 4 provides the data and the analysis sample; section 5 discusses the empirical results; and section 6 concludes.

2. Background

5.1. Public Enforcement of Regulations

Public enforcement is widely used to detect and sanction violations of laws and regulations. For example, the police detect crimes; tax auditors detect non-compliance of tax codes; and inspectors detect violations of safety, environmental, and health risks. Public enforcement is likely to have advantages over private enforcement: individuals may have limited knowledge on the identity of the violators and it is costly for the private parties to develop the technology needed to detect the violations (Polinsky and Shavell, 2007).

While the role of government agents in enforcing laws and regulations is obvious, the optimal form and level of enforcement becomes the focus of the studies on public enforcement of law. Beginning from Becker (1968), the theoretical work models the social welfare under different schemes of enforcement. In the basic framework summarized in Polinsky and Shavell (2007), individuals commit harmful activities when the gain from not complying with the regulations is greater than the expected amount of penalty for non-compliance. An increase in the frequency of inspections or the amount of penalty per violation would increase the expected costs of non-compliance, thus improve the compliance level. The social welfare is the gain of individuals from non-compliance, net the social costs of the harmful activities and the costs of detecting the violations. As an increase of the penalties for non-compliance is a costless transfer of money, the social welfare is only affected when the individuals respond by changing the level of compliance.

The empirical literature on the regulations of workplace safety examines firms' response to both changes in the frequency of inspections and the amount of penalties. Most of the previous literature finds inspections by both OSHA and MSHA ineffective in improving workplace safety (McCaffrey, 1983; Ruser and Smith, 1991; Kniesner and Leeth, 2004). The most cited reasons include the standards may not addressing the various complex causes of the accidents across different industries and the penalties for the violations are too low to incentivize firms to comply[49] (Kniesner and Leeth, 2014). This study focuses on inspections conducted by MSHA in

[49] The average penalties per violation of MSHA standard are $303 and $579 on the violation of OSHA standard.

coal mines, where the causes of accidents are similar across mines. Additionally, a flagrant violation leads to a sizable increase of total penalties, to which firms are more likely to respond.

Most previous literature also finds inspections on workplace safety are cost-ineffective (Morrall, 2003; Kniesner and Leeth, 2004). When analyzing the cost effectiveness of safety regulations, limited studies consider the potential impact on productivity.[50] Improving workplace safety may require workers to devote more efforts to preventing injuries, leading to fewer efforts on production and lower productivity. On the other hand, if firms adopt new technology to improve safety, which may also facilitate production, the productivity may increase.

5.2. MSHA Enforcement and the MINER Act of 2006

Mining has been historically one of the most dangerous industries. In 2015, the fatal injury rate in the mining sector was 11.4 cases per 100,000 full-time equivalent employees in the mining sector, more than three times as high as the 3.4 cases as the national average. The common hazards in mines include gas ignition, machinery accidents, and exposures to harmful gases, heat, and noise. In response to the high injury rate, the mining sector receives extensive regulations on workplace safety. The Mine Safety and Health Administration (MSHA), established after the Federal Mine Safety and Health Act of 1977, works to prevent death, illness, and injury from mining and promote safe and healthful workplaces for U.S. miners. MSHA is required to inspect each underground mine four times a year and each surface mine twice for occupational safety and health.

[50] One exception is Neumann and Nelson (1982), which uses aggregate data and find the Coal Mine Health and Safety Act of 1969 has no effect on the safety of the mines, but reduce worker productivity significantly.

As the most significant mine safety legislation since 1977, the Mine Improvement and New Emergency Response Act (the MINER Act) of 2006 was introduced shortly after an explosion at the Sago Mine in Sago, West Virginia in January 2006, which killed twelve mines. The MINER Act contains several provisions, regarding emergency response plans, mine rescue teams, prompt notification of mine accidents, and enhanced civil penalties. While the first three provisions focus on improving the survival of miners in disasters, the enhanced civil penalties affect the regular operation of almost every coal mine. The MINER Act increased the annual penalties assessed by MSHA substantially, from $23.2 million in 2006 to $112.3 million in 2008 (Figure 2).

The MINER Act also allows MSHA to issue citations for "flagrant" violations. A flagrant violation is "a reckless or repeated failure to make reasonable efforts to eliminate a known violation of a mandatory health or safety standard that substantially and proximately caused, or reasonably could have been expected to cause, death or serious bodily injury." A unique feature of the flagrant violation is its high financial penalty. MSHA assesses the penalty of a violation based on the history of previous violations, the size of the business, any negligence by the operator, the gravity of the violation, and the operator's good faith in trying to correct the violation promptly. Normally violations may result in fines from $112 to $70,000. In contrast, a flagrant violation could result in a penalty of up to $220,000.

The flagrant violation regime, aiming at further improving the enforcement of safety regulations, is challenged as the criteria of flagrant violations have no clear interpretation. MSHA does not provide definitions of "reckless failure", "repeated failure", "known violation", etc., making it difficult to anticipate whether a violation will be deemed as flagrant. The inspector has the initial power to issue flagrant violations, partly contributing to the large differences in the usage of flagrant violations across different MSHA districts. While fatal accidents are obviously associated with severe violations of safety standards, most of the flagrant violations are issued during a regular inspection with only five percent of the flagrant violations issued after a fatal accident. Overall, it is difficult to for the mine operator to predict whether and when a citation of flagrant violation will be issued, accompanied by a dramatic increase of penalties (Rubenstein and Blandford, 2009).

3. Empirical Strategy

The empirical objective of this study is to estimate the effect of the issuance of a flagrant violation on coal mine safety and productivity. An event study model as follows is estimated:

$$Y_{it} = \sum_{\tau \neq -1} \beta_\tau D_{it}^{\tau} + \theta_i + \delta_t + \epsilon_{it} \tag{1}$$

Y_{it} denotes the outcomes (penalties, injury rate, and worker productivity) of a mine i in calendar quarter t. Two sets of evidence are presented using equation (1). First, in graphical evidence, each period t corresponds to a quarter in a calendar year. D_{it}^{τ} is a set of period indicators that equals 1 if period t is τ quarter(s) from the quarter of the issuance of a flagrant violation and equals 0 otherwise. The model omits period $\tau = -1$, which is the quarter right before the issuance of a flagrant violation. Thus, the coefficients of interests, β_τ, represents how the outcomes change dynamically, relative to the quarter right before the flagrant violation.
For parametric estimates, the periods after the flagrant violation are grouped into short-run, medium-run and long-run, corresponded to one to two years, three to four years, and five years and after.

The identification assumption of the event-study model is that the issuance of a flagrant violation is not correlated with any pre-existing trend of the outcomes. The estimates of β_τ when

τ is negative measures changes in periods prior to the event relative to the quarter right before the flagrant violation, and provide a test on this assumption. While the coefficients after a flagrant violation reflect the response of firms to the flagrant violation, the coefficients prior to the period are expected to be close to zero.

Since mines differ in technology, type (underground versus surface), quality of coal, etc. across mines, the model includes mine fixed effects, θ_i, which control the baseline safety and productivity level of each mine. Thus, the estimates on workplace safety and worker productivity reflects within mine changes before and after a flagrant violation, and do not reflect any compositional effect. For example, mines with lower productivity or higher injury rate may be more likely to exit the market. The compositional effect is examined separately by estimating equation 1 using an indicator of mine operational status as the outcomes. The model also includes calendar year by quarter fixed effects, δ_t, to control any common shock to the industry, such as the fluctuation of coal prices over the analysis period and the general increase in penalties in 2006.Figure 3 plots the quarterly total penalties of mines with flagrant violations, with period 0 indicating the quarter of the flagrant violation. While the quarterly penalty increases steadily in periods before the flagrant violation, in the quarter of the flagrant violation, the total penalty increases substantially, from $106,000 in the quarter before to $330,000 in the quarter of flagrant violation. Thus, a citation for a flagrant violation is associated with a more than two hundred percent increase in total quarterly penalty.

4. Data and Analysis Sample

The data of this study are combined from several datasets obtained from the Mine Safety and Health Administration (MSHA), including the accident injuries data set, the quarterly employment/production data set, the inspection data set, the violation data set, the assessed violation data set, and mine addresses of record data set.

The accident injuries data set contains records on the accidents, injuries, and illnesses reported by the universe of mines. The mines report the time, location, severity of the injury, and the number of days lost or days of restricted work activity. Characteristics of the injured worker, such as age, gender, occupation, and experience are also recorded. The occupational illnesses are excluded from the analysis as occupational illnesses are mostly chronic ailment and it is difficult to determine the exact time of onset. Injuries due to natural causes, injuries involving non-employees, and injuries with missing classification code are also excluded. The data set is collapsed at the mine-quarter level, in which the number of total injuries per mine-quarter as well as the number of injuries by degree of severity are calculated, including injuries involving fatal accidents and permeant disabilities; injuries with days away from work and/or restricted work activity, injuries with no losses of work days. The quarterly injury rate is defined as the number of injuries per 100 full-time equivalent employees.

The quarterly employment/production data set includes data on quarterly coal production, total employee hours, and the average number of employees. The quarterly employment/production data set, combined with the mine addresses of record data set, which records the location and current status of the mines, is used to determine the operation status of the mines. A mine is defined as active in a year-quarter if the employment hours in the year-quarter are positive. The productivity is calculated as the number of short tons of coal produced divided by the total employee hours in a given quarter.

The inspection data set, the violation data set, and the assessed violation data set record the enforcement of MSHA. The inspection data set includes the universe the inspections conducted by MSHA. The violation and assessed violation data set recorded the type of violation and the assessed amount of penalty, if any. All the penalties are normalized to 2010 dollars values. These data sets are also collapsed at the mine-quarter level, with the quarterly number of inspections, violations, and total amount of assessed penalties calculated.

These data sets are combined using the unique mine id assigned by MSHA. The analysis sample includes coal mines from the first quarter of 2000 to the fourth quarter of 2016. The analysis sample only focuses on coal mines as the data on total production in metal/non-metal mines are not available. The coal mines with a fatal accident within one year before the flagrant violation are excluded. As fatal accidents are usually associated with extensive public attention and media coverage, they may cause changes in workplace safety and worker productivity (Gowrisankaran et al., 2017), regardless of whether the mines receive a high penalty afterwards. When a coal mine has multiple citations for flagrant violations, the first one is included. The observations with quarterly injury rates higher than 100 injuries per 100 full-time equivalent employees are excluded (0.5%). The analysis sample includes 8,133 mine-quarter observations.

Table 1 presents the summary statistics of all coal mines and separately by mines with or without a flagrant violation. The mines with flagrant violations have different observed characteristics compared with those without flagrant violations: they have relatively higher injury rates and receive more inspections and penalties. The average quarterly injury rate of mines with flagrant violations is 6.663 cases per 100 employees, almost two times as 3.375 cases, the injury rates of those without a flagrant violation. They are also larger, with the employment hours and total output around six times as those without flagrant violations. Seventy percent of the mines with flagrant violations are underground mines, which are more dangerous than surface mines due to the differences in production technology.

A control group is constructed by matching each mine with a flagrant violation to the closest mine that never received any flagrant violation. The nearest matches farther than ten kilometers are excluded. The baseline statistics of the control group is summarized in Table 1, column 4.

5. Results

5.1. Workplace Safety

As a flagrant violation is associated with a substantial increase in penalties, based on the theory of public enforcement, it should lead to better compliance with the safety regulations. The graphical evidence on the effect of the flagrant violation on workplace injury rates are presented in Figure 4. The graph shows the estimates of β_τ from equation 1, with the standard errors clustered at the mine level and the vertical bands reporting the 95% confidence interval. The workplace injury rate decreased from the fourth quarter after the issuance of a flagrant violation, and the decreasing trend persisted till twelve quarters after. The coefficients of β_τ in periods prior to the flagrant violation are close to zero and statistically insignificant. These estimates suggest that during the three year period right before the flagrant violation, the injury rate is not statistically different from the injury rate in the quarter before the flagrant violation, which implies that the issuance of flagrant violations is not precipitated by a pre-existing increasing trend of workplace injuries.

Table 2 presents the estimation results, with the period indicators grouped into one to two years after the flagrant violation, three to fours years after, and five years and after. The injury rate decreased by 0.182 cases per 100 employees during the first tow years after a flagrant violation. Compared to the average total case rate as 7.0 cases in the quarter right before the flagrant violation, the effect represents a 3 percent decrease. The injury rate continues to decrease in the medium- and long-run. Between year three to year four after the flagrant violation, the injury rate decrease by -1.388 cases per 100 employees (20%).

To examine the composition of the decrease in injury rates, the injuries are divided into three categories based on the severity, including fatal and permanent injuries, injuries with days away or restricted from work, and injuries with medical treatment but no losses of workdays. The graphical evidence is presented in Figure 4, Panel B-D. The results suggest that the decrease of workplace injuries almost exclusively come from the decrease of injuries with days away or restricted, which drop by 0.542 cases during the first two years after the flagrant violation and by 1.400 cases between year three and year four after (Table 2, column 2). The fatal and permanent injuries and mild injuries show small and statistically insignificant changes both before and after the flagrant violation (Table 2, column 3 and 4).

One possible mechanism of the decreased workplace injuries is through reduced working intensity. Previous studies have shown longer working hours and higher working intensity are associated with more workplace injuries and higher health risks (Ruhm, 2000; Hummels, Munch, and Xiang, 2016). To test this hypothesis, Figure 5 shows the impact of a flagrant violation on quarterly working hours per worker, defined as quarterly total employee hours (in 1,000) divided by employee count. The quarterly working hours per work decreased right after the issuance of a flagrant violation, and continued decreasing over a three-year period. The estimates are presented in Table 3, column 1. In the medium run, the average hours decrease by 23 hours per worker per quarter, representing a 4% decrease, compared to the average 568 hours per worker per quarter..

The decrease of the injury rates is unlikely to be driven by the negative financial shock from the penalties. First, the average quarterly production of the mines is 547 thousand short tons. With the average price of coals as $56, the increase of $0.22 million penalties accounts for less than 1 percent of the quarterly revenue of the mines. Second, the previous studies on the effect of a financial shock on workplace safety and compliance with regulations find that a negative financial shock is likely to increase injuries and reduce compliance of regulations (Cohn and Wardlaw. 2016; Earnhart and Segerson, 2012), which contradicts the results that injury rates decrease after a flagrant violation.

5.2. Productivity

It is clear that flagrant violations are associated with a sizable and persistent decrease of workplace injuries in coal mines. Such improvement of workplace safety may require workers to devote extra effort to preventing injuries, thus negatively affect worker productivity. Figure 6 presents the effect of flagrant violations on worker productivity, defined as the number of short tons of coals per employee hour. The graph shows that productivity decreases right from the issuance of a flagrant violation and the decreasing trend persisted over a three-year period. The coefficients in periods before the flagrant violation are close to zero and statistically insignificant, which suggest that no pre-existing trend of productivity led up to the issuance of the flagrant violation.

Column 2 of Table 3 presents the estimated coefficients. In the period of flagrant violation, the productivity decreases by 0.247 short tons of coal per labor hour, representing a five percent decrease. Three years after, the magnitude is 0.322 tons of coal per labor hour (7%).

The introduction of flagrant violations led to improved workplace safety conditions but lower worker productivity. Using the coefficients obtained from the event study model, the net benefits of the regulation is examined. In the medium run (three to four years) after the issuance of a flagrant violation, the quarterly workplace injury rates decrease by 1.388 cases. The average hourly wage of coal miners. With the estimated costs of one non-fatal injury being $39,520 (National Safety Council, 2015), it suggests a cost saving of $1.10 per labor hour (1.388 *39,520 /50,000). The productivity loss, which is 0.322 ton per hour of labor input one year after flagrant violation, amounts to $1.46 per hour (25.75*0.322/5.689) with the average hourly wage of mines as $25.75 and average labor productivity as 3.18 short tons per hour. Thus, the losses from lower worker productivity are 1.3 times the gains from improved workplace safety.

5.3. Mine Closures

The analysis on workplace safety and worker productivity above uses the event-study model with mine fixed effects, which estimates changes within mines before and after the flagrant violation. Beyond the direct effect on mine safety and productivity, the flagrant violations may also affect mine exits. For example, more dangerous mines or less productive mines may be affected disproportionally after a flagrant violation and more likely to exit.

Figure 7 presents the effect of flagrant violations on the closure of mines. A coal mine is active in a corresponding quarter if it has positive employment hours. Conditional on being active in the previous quarter, the mine exit variable equals 1 if a mine is active in the corresponding quarter and equals 0 otherwise. The likelihood of exit increased right after the flagrant violation and persists over a three-year period. During the first two years after a flagrant violation, a mine is 3 percentage points more likely to exit the market (Table 3, column 3). In quarters before the flagrant violations, the coefficients are small and insignificant.

To understand how the increasing mine exits affect the aggregate trend of mine safety and productivity, the analysis sample is separated into mines with productivity above or below the median and mines with employment hours above or below the median, measured as of the quarter before the flagrant violation. The estimates are presented in Table 4. The increasing mine exits are predominantly driven by the smaller mines and the less productive mines. Thus, when simply comparing the aggregated trend of productivity before and after flagrant violations, the effect on individual mines is likely to be underestimated as the less productive mines exit after the flagrant violations.

5.4. Robustness Check

The main results estimated using the event study model assumes that no other shock existed during the same time as the flagrant violation. To test this assumption, each mine with a flagrant violation is matched to a closest coal mine without any flagrant violation. The changes in safety and productivity is tested among the matched coal mines before and after a flagrant violation in a nearby mine. Figure 8 presents the graphical evidence. Both the injury rate and worker productivity show small and insignificant change before and after the flagrant violation.

While the estimates using the event study includes all the mines that have ever received a flagrant violation between 2006 and 2016, twenty-one percent of mines closed within two years of the flagrant violation. Figure 9 presents the graphical evidence of a flagrant violation among mines without any change in operational status within two years before and after the flagrant violation. The results are similar to those of the main analysis sample: both workplace injury rate and worker productivity showed small and insignificant change before the flagrant violation and decreased persistently after.

6. Conclusion

This study examines the effect of high-penalty flagrant violations on coal mine safety and miner productivity. The results highlight the trade-off between workplace safety and worker productivity: after a flagrant violation, the workplace injury rate decreased while the worker productivity also decreased. The likelihood of a mine closure increased by 4 percentage points during the first two years after the flagrant violation.

While public enforcement is widely used in regulating health and safety risks, most studies focus on its effectiveness in reducing the targeting risks and often overlook the potential costs on production losses and plant exits. The results of this study imply the value of the productivity loss is 30 percent higher than the gains from reduced injuries, suggesting that omitting the costs of productivity loss will substantially overestimating the benefits of safety regulations.

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Table 1. Summary Statistics

	Whole Sample	Flagrant Violations	No Flagrant Violations	No Flagrant Violations, Closest Match
Inspections	2.451	8.773	1.912	3.204
	(4.266)	(9.840)	(2.793)	(5.681)
Penalties ($1,000)	6.818	56.633	2.573	5.497
	(54.423)	(181.932)	(12.814)	(21.735)
Violations	9.317	46.845	6.119	11.861
	(22.867)	(54.912)	(13.426)	(24.309)
Injury Rate	3.634	6.663	3.375	4.935
	(9.727)	(9.510)	(9.702)	(11.271)
Injury Rate: Permanent	0.044	0.106	0.038	0.077
	(0.946)	(1.107)	(0.930)	(1.107)
Injury Rate: Days Loss	2.590	4.579	2.420	3.462
	(8.084)	(7.702)	(8.093)	(9.605)
Injury Rate: Mild	1.000	1.978	0.917	1.396
	(4.734)	(4.579)	(4.738)	(5.334)
Employment Hours (1,000)	22.899	94.594	16.788	31.723
	(49.334)	(113.285)	(32.744)	(68.570)
Coal (1,000 Short Tons)	130.176	546.920	94.658	201.203
	(800.614)	(2180.924)	(523.661)	(974.135)
Underground	0.274	0.718	0.236	0.336
	(0.446)	(0.450)	(0.424)	(0.472)
Surface	0.448	0.161	0.473	0.249
	(0.497)	(0.368)	(0.499)	(0.432)
Facility	0.278	0.121	0.291	0.415
	(0.448)	(0.326)	(0.454)	(0.493)
Observations	103,561	8,169	95,428	3,884

Note: Data are quarterly observations from Mine Safety and Health Administration (MSHA) 2000-2016.

Table 2. The Effect of Flagrant Violation on Workplace Safety

	Injury Rate	Injury Rate-Days away or Restricted	Injury Rate-Permanent	Injury Rate-Mild
year1to2	-0.182	-0.542	0.030	0.331
	(0.535)	(0.439)	(0.031)	(0.256)
year3to4	-1.388***	-1.400***	-0.018	0.029
	(0.487)	(0.400)	(0.024)	(0.228)
year5after	-1.440**	-1.391**	0.054	-0.103
	(0.717)	(0.573)	(0.040)	(0.329)
R2	0.058	0.051	0.013	0.022
N	8,133	8,133	8,133	8,133

Note: Data are quarterly observations from Mine Safety and Health Administration (MSHA) 2000-2016. The injury rate is measured as the number of workplace injury cases per 100 full-time equivalent employees.

Table 3. The Effect of Flagrant Violation on Working Hours and Worker Productivity

	Hours per Worker	Productivity	Survival
year1to2	-0.006	-0.247**	-0.030***
	(0.007)	(0.101)	(0.010)
year3to4	-0.023**	-0.322**	-0.028**
	(0.009)	(0.132)	(0.012)
year5after	-0.003	-0.312	-0.043***
	(0.012)	(0.255)	(0.014)
R2	0.049	0.179	0.023
N	8,133	8,133	8,035

Note: Data are quarterly observations from Mine Safety and Health Administration (MSHA) 2000-2016.

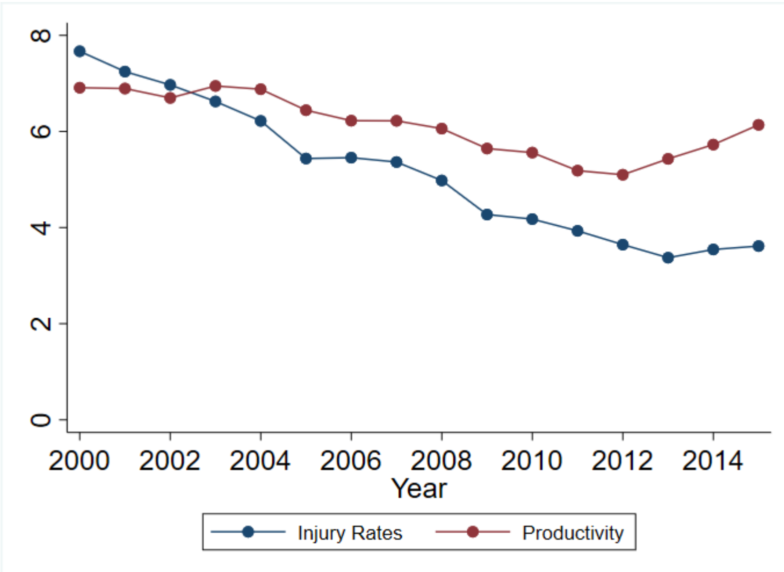


Figure 1. Injury Rates and Miner Productivity in Coal Mines, 2000-2015

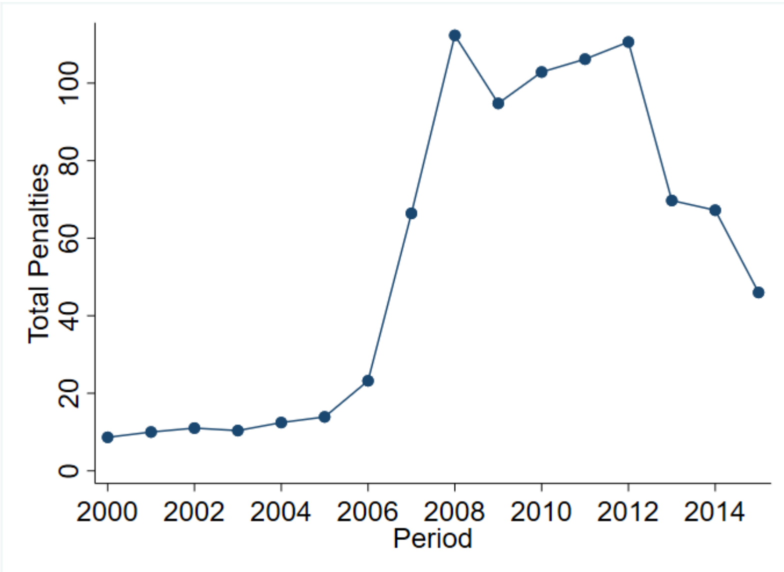


Figure 2. MSHA Penalties on Coal Mines, 2000-2016

Note: Data are from Mine Safety and Health Administration (MSHA).

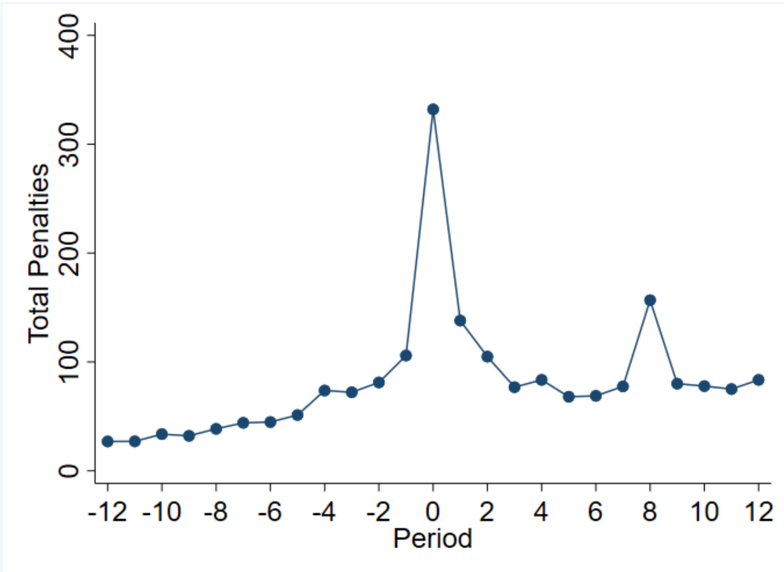
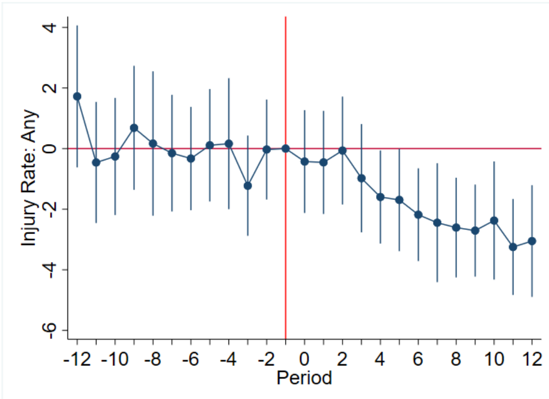


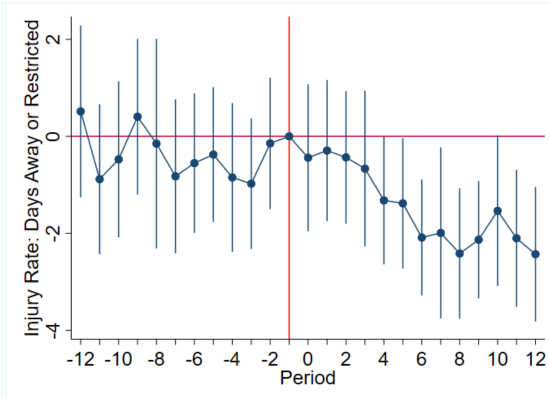
Figure 3. Impact of Flagrant Violations on Total Penalties

Note: Data are from Mine Safety and Health Administration (MSHA). Period 0 indicates the quarter of the flagrant violation.

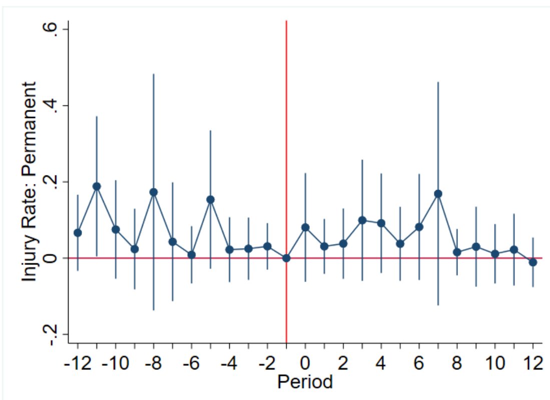
Panel A. Any Injuries



Panel B. Days Away or Restricted



Panel C. Fatal or Permanent



Panel D. Mild

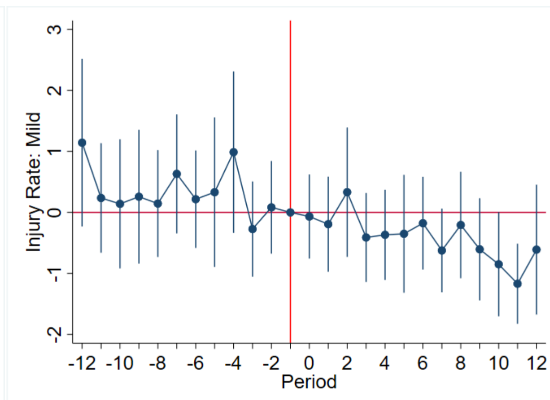


Figure 4. Impact of Flagrant Violations on Injury Rates

Note: Data are from Mine Safety and Health Administration (MSHA). Period 0 indicates the quarter of the flagrant violation.

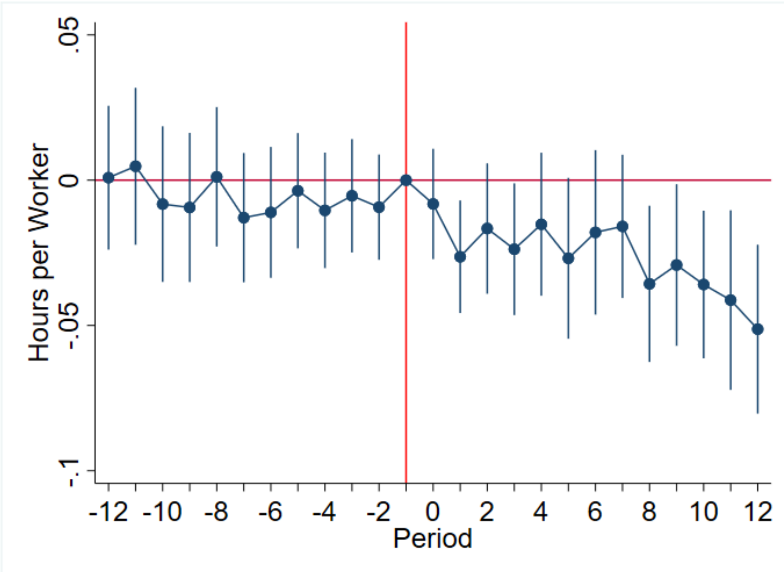


Figure 5. Impact of Flagrant Violations on Hours per Worker

Note: Data are from Mine Safety and Health Administration (MSHA). Period 0 indicates the quarter of the flagrant violation.

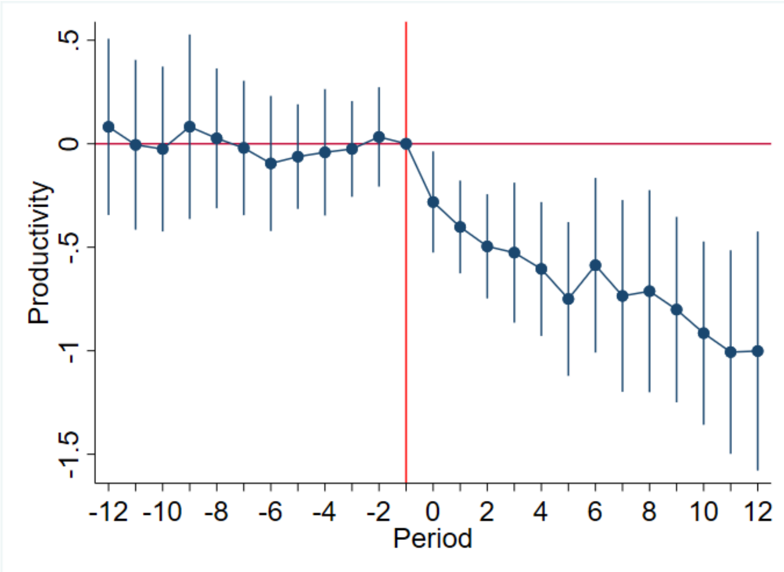


Figure 6. Impact of Flagrant Violations on Worker Productivity

Note: Data are from Mine Safety and Health Administration (MSHA). Period 0 indicates the quarter of the flagrant violation.

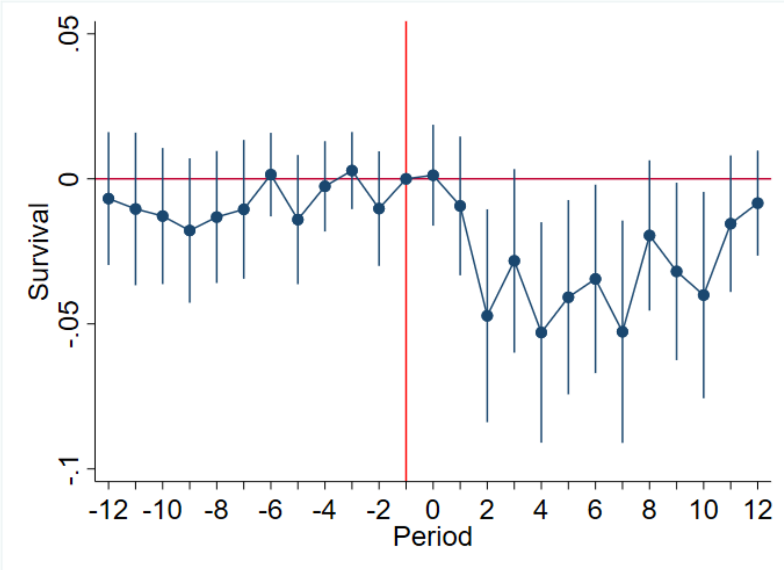
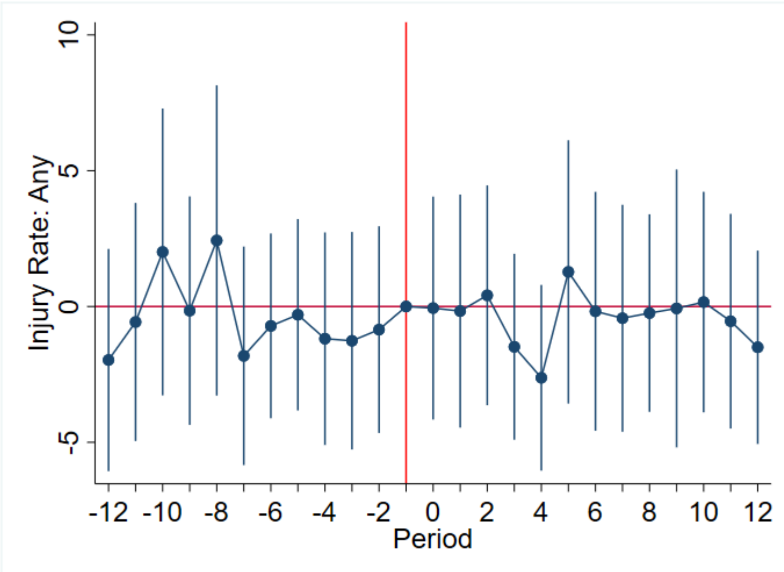


Figure 7. Impact of Flagrant Violations on Operation Status

Note: Data are from Mine Safety and Health Administration (MSHA). Period 0 indicates the quarter of the flagrant violation.

Panel A. Any Injuries



Panel B. Worker Productivity

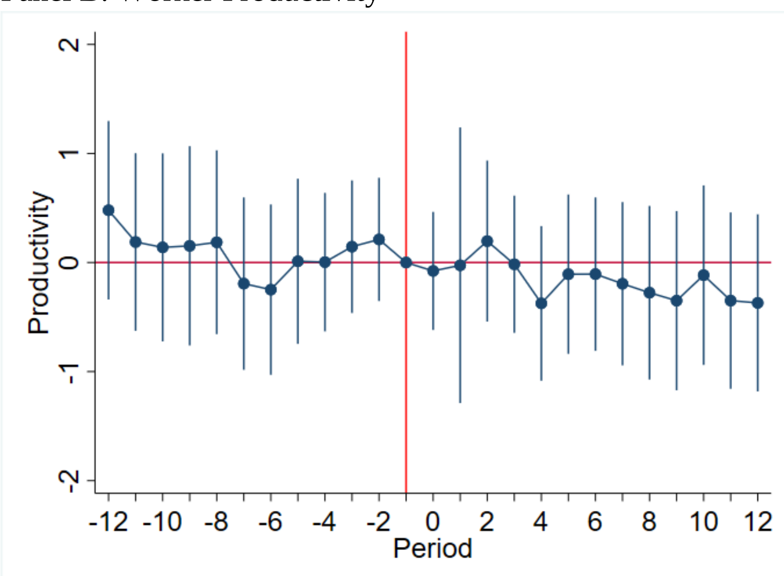
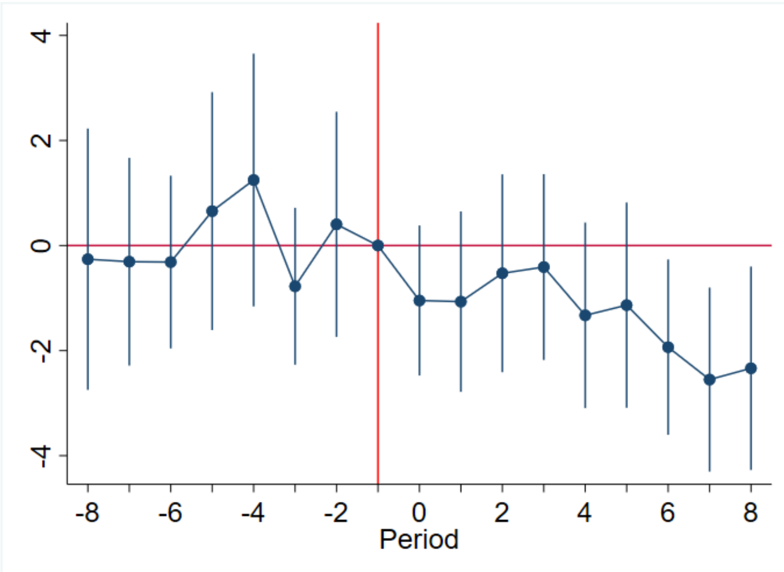


Figure 8. Impact of Flagrant Violation on Safety and Productivity, Comparison Group

Note: Data are from Mine Safety and Health Administration (MSHA). Period 0 indicates the quarter of the flagrant violation.

Panel A. Any Injuries



Panel B. Worker Productivity

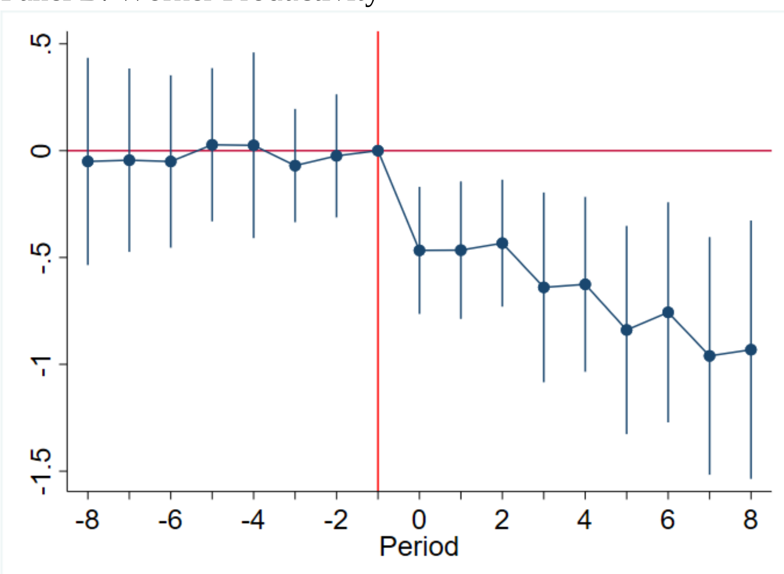


Figure 9. Impact of Flagrant Violation on Safety and Productivity, Mines Active during the Analysis Period

Note: Data are from Mine Safety and Health Administration (MSHA). Period 0 indicates the quarter of the flagrant violation.

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2010	Tsinghua University Yang Shaowei Scholarship
2009	Tsinghua University Freshman Scholarship